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THE QUARTERLY REVIEW
*of* BIOLOGY

# WAS ARISTOTLE AN EVOLUTIONIST?

BY HARRY BEAL TORREY AND FRANCES FELIN
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A CRITICAL re-examination of this question has been prompted by the large differences of opinion that have been found to exist among the historians and commentators on whom the reading public has been accustomed to rely for its own opinions of Aristotle. His extraordinary influence on the course of western civilization—an influence still very much alive, often conspicuous, in the intellectual life of today—gives his views on any subject peculiar importance. Doubly desirable is it to be clear as to his attitude toward so significant and pervasive an influence in the modern world as the evolution idea. This has provided the motive for the present attempt to find, in spite of current misunderstandings, whatever unity the facts themselves may warrant. To reach this goal, it has seemed profitable, first, to consider the senses in which the term evolution is commonly used; next, to summarize contemporary opinion; finally, to let Aristotle speak for himself.

## I

Among the many meanings that have been given to the term evolution since its introduction into biology in 1762 by Charles Bonnet (24), there are at least four that may well be kept in mind as we pursue our present enterprise:

*Evolution as the development of the individual.* This was as Bonnet used it. But he conceived development, on philosophical grounds, as an unrolling or unfolding of a germ that contained, and had contained from the instant when germs were first created, the counterpart in miniature of every characteristic destined to appear in the adult. It was thus a mere extension of a pre-formed organization by a sort of interstitial swelling of pre-existing elements. The term evolution was chosen originally to fit this particular conception of individual development. With the rise of embryology, however, in the first half of the nineteenth century and the total eclipse of Bonnet's special views, it was transferred from the latter to the more general case, becoming easily identified with the "succession of gradual



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changes" by which the egg "evolved" into the adult, whatever the underlying mechanism.

"These successive changes of growth," wrote Louis Agassiz (3), just before he died, "constitute evolution, as understood by embryologists, and within these limits all naturalists who know anything of Zoology may be said to be evolutionists. The law of evolution, however, so far as its working is understood, is a law controlling development and keeping types within appointed cycles of growth, which revolve forever upon themselves, returning at appointed intervals to the same starting-point and repeating through a succession of phases the same course. These cycles have never been known to oscillate or pass into each other."

*Evolution as the development of the race.* This was to Agassiz, writing in 1873, a "recent and novel application" of the term *evolution*. As early as 1852 Spencer (53) had used Bonnet's term in this new sense. While the last word in the first and every subsequent edition of the *Origin of Species* (27) is "evolved," it is not until the sixth edition, finished in 1872 and published ten years later, that Darwin extends this meager usage by mentioning "evolution" three times in one paragraph of the concluding chapter. It was Darwin's custom to speak of "descent with modification" when he wished to recall that races, like individuals, change as they develop, and that racial types are not of necessity permanent but show variations that are perpetuated, accumulated, modified, from generation to generation. His expressive phrase, however, with its explicit recognition of the genetic factor, was soon to give way to its less colorful and definitive successor. When Agassiz wrote, *evolution* had become familiar to English ears in its new meaning.

*Evolution as the development of the cosmos.* This application of the term *evolution* in its most inclusive and general form was also, in its beginnings, Spencer's doing (55, i, 201 ff). From boyhood, in his contemplation of nature, he had sought interpretations untinged with supernaturalism. When, therefore, in 1840, he first met Lamarck's views on the mutability of species in the pages of *The Principles of Geology* (38), he found in them a "congruity with the course of procedure throughout things at large" that gave them, in spite of Lyell's unfavorable comment, "an irresistable attraction." Two years later, in a series of letters to *The Non-Conformist* on *The Proper Sphere of Government*, he showed a recognition of "certain factors in the process of evolution at large." The reflections thus initiated led to *Social Statics* (1850), and this to *The Principles of Psychology* (1855). Then, early in 1858, in a mind gradually saturating with such studies, there suddenly crystallized an outline of the *System of Synthetic Philosophy*. This was only a few months before that memorable meeting of the Linnaean Society when Darwin (28) broke his long silence and *The Origin of Species* at last was born.

There is no need to pause here over the abstract terms of the well known definition of evolution which Spencer (54) now applied as a basic formula to Nature in all its manifestations—inorganic, organic, and, as he said, superorganic. It was essentially a philosophic summary, a least common denominator; reminiscent, too, of kindred speculations by the naturalists of ancient Miletos who first expressed the conception of a mechanical, as well as a changing, universe that made natural science possible. In so general a form it was intended to include but not to replace the more restricted conceptions of evolution which, as has already been indicated, were associated with various special aspects of the general problem.

*Evolution as the development of human culture.* Under this head fall naturally

the manifold results of human activities, such, for instance, as: social, political, religious institutions; languages; the arts and sciences, with their applications to architecture, industry, medicine, agriculture, domestic life. All these may be studied as to their development and evolution. But the words now take on new meanings, with the shift of interest from organic processes to their products, from man himself to that not inconsiderable portion of his environment which he has fashioned and in which he is forced to live. Change in this vast artefact is evolution of a sort, but without hint of organic reproduction. Close as may be resemblances between cathedrals, or airships, or philosophies, it is as the handiwork of man the artificer that they are related, not by anything remotely suggesting Darwinian descent with modification. Of this Spencer was fully aware when, in his Prospectus (54: xvi) distributed in 1860, he specifically characterized as evolution the development of governments, languages and ideas.

## II

Was Aristotle an evolutionist? Since the convenient and accessible outline of the history of the evolution idea published forty years ago by H. F. Osborn (43), there have been numerous answers to this question, with a variety of interpretations amounting in extreme cases to diametrically different conclusions. These will be examined in the present section, as a preliminary summary of current opinion.

Osborn himself believed that Aristotle "was the first to conceive of a genetic series, and his conception of a single chain of evolution from the polyps to man was never fully replaced until the beginning of this century" (p. 44). Farther on, he arranged in five sections adaptations of several passages from Book 2 of the *Physics* as translated by T. Taylor (5). In the last of these, taken from Chapter 8, are the following words: "*Further still, it is necessary (i.e., according to law) that germs should have been first produced, and not immediately animals; and that soft mass which first subsisted was the germ.* . . Nature produces those things which, being *continually moved by a certain principle contained in themselves*, arrive at a certain end" (p. 56). His conclusion, following immediately, is of especial significance: "These passages seem to contain absolute evidence that Aristotle had substantially the modern conception of the Evolution of life, from a primordial, soft mass of living matter to the most perfect forms, and that even in these he believed Evolution was incomplete, for they were progressing to higher forms" (p. 57). Osborn refers here to an "'internal perfecting tendency', driving organisms progressively forward into more perfect types" (p. 51).

Certain comments at once suggest themselves. In the first place, as has already been indicated, one may arrange objects in a series designed to emphasize their resemblances without thereby committing himself to a belief in their blood relationship. There could be, obviously, no blood relationship between golf balls, however minute their differences; or between locomotives, as Lewes (34: 189) long ago remarked. Such an inference would be hardly less superfluous in the case of organisms, in the absence of a statement expressly to the contrary—which has thus far eluded us. Whether or not he ever gave it a thought is a question that will be considered later. For the present, it seems necessary only to say that if his series, whose terms are coexistent in time, is to be regarded as in any sense a genetic sequence, this conclusion must rest on evidence other than the mere existence of the series itself.

Similar reasoning is applicable to the assumption that Aristotle's well known belief in an 'internal perfecting tendency' in nature carries with it a belief in the transmutation of specific types.

Finally, it should be said that whatever may have been Aristotle's views on the "Evolution of life, from a primordial, soft mass of living matter," the text of Book 2 of the Physics does not reveal them. The words that Taylor rendered "that soft mass which first subsisted" are the words, not of Aristotle, but of Empedocles. In the Greek text translated by Wicksteed and Cornford (6), they appear in quotation marks: '*oulophues men prota*,' and their derivation is expressly indicated in this rendering by the introduction of the name of Empedocles, thus: "Empedocles' 'primal all-generative'" (199b, 9). They come from one of Empedocles' Fragments, to be found in Fairbanks (30) in both Greek and English as No. 262, and in Burnet (25) in English as No. 62. Fairbanks translates them: "In the first place . . . forms grown into one whole:" (p. 191) and Burnet: "Whole-natured forms first" (p. 249); and more recently, Hardie and Gaye (9), following Burnet: "whole natured first." None of these translations suggests the "soft mass" of Taylor, nor does the Greek text embody this or any other description of the physical character of the *sperma*. Furthermore, Aristotle's quotation of Empedocles' phrase is merely incidental in a chapter that is concerned with a discussion of *purpose in nature*, not at all with theories of the origin and evolution of life.

E. Clodd (26), who appears to have been indebted exclusively to Osborn for his account of Aristotle's "pioneer work in evolution," recurs to "the question of the origin of life forms" with the assertion that Aristotle "was nearest of all to its modern solution, setting forth the necessity 'that germs should have been first produced, and not immediately animals; and that soft mass which first subsisted was the germ'" (p. 18). There is no need to comment further on this misconception.

W. A. Locy (35), a decade later, writes as follows: "Professor Osborn in his interesting book, *From the Greeks to Darwin*, shows that Aristotle had thought out the essential features of evolution as a process in nature. He believed in a complete gradation from the lowest to the highest, and that man is the highest point of one long and continuous ascent" (p. 12).

The words: "thought out the essential features of evolution as a process" are Osborn's (p. 50). Osborn had just been discussing "Aristotle's conception of movement, as outlined in his Physics" (Bk. 3, Ch. 1). His account appears to be taken in part from Zeller (65). But neither in Zeller nor in the Greek text (6) nor in the translations of Taylor (5), Wicksteed and Cornford (6), and Hardie and Gaye (9), is there the suggestion of a primarily biological motive for the passage or a basis for the alternative meanings which he has supplied, in biological terms, for the four kinds of movement there characterized—with the possible exception of the first.

Further consideration of this reference to Aristotle's views will be found in the literature already cited. It should be said that Locy (36), during the seventeen years following the publication of his first book, found reasons for restating his view with much subdued conviction: "In the philosophical consideration of animal life, Aristotle possibly glimpsed the germ of the idea of evolution" (p. 32).

This was some years after Royce (48) had written his illuminating essay on Herbert Spencer in which he said: "Aristotle did indeed lay great stress upon the

evolution everywhere present in the sublunary region of 'genesis and corruption', but in this region it was each individual thing which grows and then passes away. The 'forms' which are responsible for the evolution of individuals are as eternal as the Platonic ideas. They therefore do not evolve" (p. 28). And again: "This conception of the *eternity of the forms of things* is, historically considered, by far the most significant opponent that the philosophical doctrine of evolution has ever had or ever can have" (p. 29).

Arthur Platt (14) makes an interesting note—here reproduced only in part—on the discussion by Aristotle of the possible 'earth-born' origin of men and quadrupeds: "I think it certain that the transmutation of species in any modern sense no more occurred to him than to Empedocles. He contemplates the possibility that man's ancestor was a scolex; he never thought that he might have been a monkey. Each species would have had a separate beginning by spontaneous generation; they would not be related by descent from a common ancestor" (762b, 29, note 2).

T. E. Lones (37), commenting on the statement of Aristotle (19) that "Some animals share the properties of man and the quadrupeds, as the ape, the monkey and the baboon" (502a, 17) says: "Neither in this nor, apparently in any other passage does Aristotle show that he had any idea of a development of higher forms of life from common ancestors, at all resembling the Darwinian idea of the origin of species" (p. 82).

Two years later, A. W. Benn (23) inclusively and crisply asserts:

> "He did not, like the Ionian physiologists, anticipate in outline our theories of evolution. He held that the cosmos had always been, by the strictest necessity, arranged in the same manner; the starry revolutions never changing; the four elements preserving a constant balance; the earth always solid; land and water always distributed according to their present proportions; living species transmitting the same unalterable type through an infinite series of generations." (p. 272)

Charles Singer (51) does not believe it can be claimed that Aristotle "regarded the different kinds of living things as actually passing one into another but there can be no doubt that he fully realized that the different kinds can be arranged in a series in which the gradations are easy" (p. 29). However, he says in a later publication (52):

> "It is probable that in ascribing to animals certain human qualities, Aristotle was influenced by his advance toward what would nowadays be called a belief in Evolution. . . . It cannot be said that he ever definitely attained to the 'evolutionary' point of view. But it is evident that he was moving in that direction, and perhaps if he had lived another ten years he might have reached it. But, whether we call him an Evolutionist or whether we deny him that title, it is yet quite easy to read an evolutionary meaning into some of his biological writings. To do this is to develop but not to force his meaning" (p. 39).

Less tentatively, *G. Sarton* (49) writes that Aristotle "outlined the theory of evolution (scala naturae) and a scientific classification of animals" (p. 128).

A. E. Taylor (57), however, believes that Aristotle "turned his back on evolutionary ways of thinking" (p. 144). Why, he asks, was Aristotle "content with applying the notion of biological development to the growth of the individual. Why did he not extend it to the kind?" (p. 446).

H. H. Newman (41) appears to have thought that he did. For in Newman's opinion, the

> "evolution idea took a great step forward with Aristotle and reached a stage beyond which it did not go for many centuries . . . He . . . attributed all evolutionary changes to natural causes. . . . He 'had substantially the modern conception of the Evolution

of life, from a primordial soft mass of living matter'. . . . He had an idea of a linear phylogenetic series, beginning with plants, then plant-animals, such as sponges and sea anemones, then animals with sensibility, and thence by graded stages up to Man" (p. 13).

These passages, reminiscent as they are, call for no further comment at this time—unless it be the observation that after more than thirty years it is to Osborn and not to Aristotle himself that writers of widely read textbooks continue to acknowledge their indebtedness for materials that Aristotle alone could have known at first hand.

E. Nordenskiöld (42) is, with one exception (Singer, 52) the most recent of the authors who have helped us to determine Aristotle's position as an evolutionist in the current of informed opinion. Under the caption: "The first evolutionist," he writes:

"Here we find enunciated for the first time a really complete theory of evolution. . . . Aristotle saw a consistent evolution from lower to higher forms of being, and although it is based on purely metaphysical speculation, this idea has proved for all time a fertile one in the biological sphere, for the very reason that it is here in agreement with actual fact" (p. 37). Again: "The finest merit of this system of thought lies in the fact of its being based on an evolution subject to rigid laws and proceeding from the lower to the higher. But as this theory of evolution is, as has been shown above, primarily based on a predominant guiding intelligence, it acquires a dogmatic arbitrariness: the subjection to law is not an act of nature itself, but rather the product of divine wisdom, or, in other words, human speculation. . . . Thus it came about that . . . the man who was the first to introduce and logically to apply to the conception of the entire universe a theory of evolution from the lower to the higher appeared fifteen centuries later as the founder of a system of stagnation and obedience to authority." (p. 43)

Inadequate as these quotations are, they reveal once more an association of the evolution idea with a *scala naturae*. But they leave one in some doubt as to whether the theory of evolution which, according to Nordenskiöld, Aristotle applied logically to "the entire universe" may not be rather a principle of logical classification than a recognition of causal succession in time.

This ends our summary of current opinion.

What, now, does Aristotle say for himself?

## III

Two centuries before Aristotle was born, the speculative foundations of science were laid by the hardy school of natural philosophers that Thales founded at ancient Miletos. It is not necessary here to try, out of the fragmentary records of that day, to give definite shape to individuals or the authority of personal reference to their views. What Thales, Anaximander, Anaximines and their successors throughout the Greek world contributed as a whole to cosmology during the sixth and fifth centuries before Christ will provide a general background for the views of Aristotle with which we are especially concerned (See esp. Fairbanks, Burnet, Bailey, Benn).

First of all, they conceived the universe as a self-contained mechanism, without design or purpose, ruled by necessity from within, not by a Divinity or other agency from without. So doing, they broke boldly from the past, initiating a profoundly significant revolution in human thought. They conceived also the universality of change, and a fundamental substance infinite in extent, out of which the multiform world of their experience had differentiated, out of which indeed countless worlds had arisen and decayed through infinite time, in cycles that carried throughout the universe the conception of recurrent change so familiar to their mundane experience.

Thus, in sweeping speculations, they

sketched a developing universe, made mechanically intelligible by the partition of the basic substance into ultimate particles, which varied in size and shape but were eternal and indestructible as the stuff of which they were composed. They were also inherently active, capable of moving in all directions in a structureless void. It was from the movements of these atoms, governed by necessity but without purpose or plan, that aggregates, infinite in number and variety, shaped themselves into earth and sun and moon and stars and all other objects whatsoever, great and small.

Aristotle was not unfamiliar with these evolutionary views. It was not from ignorance but from choice that, under radically different influences he devised a radically different cosmos. Long before his birth Miletos had ceased to exist as a center of philosophical culture. Educated as a boy at the Macedonian court in the medical tradition of his father Nicomachus, he was sent as a matter of course at seventeen to Athens to study philosophy at the Academy, where he began what proved to be an intimate association for twenty years with his master Plato. There, during the latter's absence in Sicily, he fell under the potent charm of the earlier Plato of the Dialogues. There for twenty years he breathed an atmosphere charged with Platonic forms, final causes and teleological interpretations. There he lived in a Platonic world of imperfections, the shadows and images of the eternal realities that existed, detached and perfect, in heaven alone.

His *cosmology* was a product of this early Platonic period, when he still wrote as a true disciple of the master, before an increasing preoccupation with the tangible problems of the sublunary world of experience determined the final focus of his interests. The time came when he criticized Plato; but whatever his criticisms, he remained, in comparison with his Ionian forbears, a Platonist to the last. For him as for Plato, the eternal heaven (*De Caelo*, 283b, 26; 287b, 25) was the abode of divine perfection (*De Part. An.*, 644b, 21). It encompassed a finite universe (*De Caelo*, 273a, 5; 276a, 18) in the form of a sphere (286b, 10) of all forms uniquely capable of eternal motion (*De Gen. et Corr.*, 337a, 1; 338a, 18ff) and with definiteness (*P. A.*, 641b, 23) demanded by a world in which "Nature always strives after the better" (*G. C.*, 336b, 27; *De Incessu*, 704b, 15; *De Juv. et Sen.*, 469a, 29). Embedded in this outermost sphere were spherical stars (*De Caelo*, 289b, 31ff; 290a, 8; 291a, 27; 291b, 11); and within it, other spheres for the other heavenly bodies; and between these still other spheres whose function it was to account for certain apparent motions of the bodies themselves (291a, 30ff; *Metaph.*, 1074a). All the spheres were of ether, the finest of the elements as it was also, from the earth, the most remote. All were in contact. And they revolved regularly and invariably through endless time around the earth, which was fixed and motionless at the center (*De Caelo*, 286a, 21; 286b, 9; 296b, 21). Thus Aristotle conceived a universe that was uncreated, changeless, eternal. Its material substrate was a continuum (*Phys.*, 216b, 20ff), in which the four (five) elements existed as fundamental substances, not ultimate atomic particles (*G. C.*, i, 8; *De Caelo*, 303a, 3ff). And motion, instead of emanating from the latter, was communicated to the outermost of the spheres by an unmoved Prime Mover, without substance or mechanical support (*Phys.*, 267b, 15; *Metaph.*, 1072a, 24; 1074a, 35) existing apparently only as a metaphysical device (*G. C.*, 337a). Thence, from sphere to sphere, motion proceeded to the sub-

lunary world, whose individuals exhibited the well-known changes of growth and decay with their attendant variations and material imperfections not observable in the distant and inaccessible heavens (*De Caelo*, 269b, 16). Form, however, was to Aristotle more significant than matter (*P. A.*, 640b, 29; *Ph.*, 193a, 30; 194b, 10; 200a, 32; *Metaph.*, 1029a, 5; 1041b, 8); formal and final causes were dominant factors in the cosmic mechanism. Sublunary individuals might change materially, but their forms—including their specific types—were fixed and without beginning or end (*De Gen. An.*, 731b, 35; *De An.*, 415b, 3).

It is clear that such a universe, rigid, uncreated and immutable, left no opportunity for the ceaseless differentiation of which the plastic universe of the Ionians was capable. Whatever Aristotle's understanding of the ways of terrestrial nature, it does not seem possible, by the farthest stretch of the imagination, to reckon him among the cosmic evolutionists.

In the sublunary world, indeed, he was more at home. It was tangible and could be investigated objectively by the observation of individuals, to which he turned more and more in the last great productive years of his life. In the *Meteorologica* (see especially i, 14) he discussed *changes in the earth's surface* as incidents in the development and decay of its parts analogous to similar cycles in the lives of organisms. Among these terrestrial changes, the phenomena of sedimentation and erosion especially interested him. He regarded the whole country of Egypt as "a deposit of the river Nile" (*Meteorol.*, 351b, 28). He called attention to the fact that

"in the time of the Trojan wars the Argive land was marshy and could only support a small population, whereas the land of Mycenae was in good condition (and for this reason Mycenae was the superior). But now the opposite is the case, for the reason we have mentioned: the land of Mycenae has become completely dry and barren, while the Argive land that was formerly barren owing to the water has now become fruitful. Now the same process that has taken place in this small district must be supposed to be going on over whole countries and on a large scale" (352a, 9–16). "But the whole vital process of the earth takes place so gradually and in periods of time which are so immense compared with the length of our life, that these changes are not observed, and before their course can be recorded from beginning to end whole nations perish and are destroyed" (351b, 8).

These passages have a modern sound. With all his interest in sedimentation, however, Aristotle seems never to have understood, as his predecessor Xenophanes did, the nature of fossils. And since he could not have known the records left by successive organic types and only recently brought to light by the combined efforts of many palaeontologists, his imagination remained quite untouched by the vast panorama of geologic history that is a portion of our present inheritance.

Of *gradual transformations in human institutions* through long periods of time, he was made aware by the intensive historical studies of his later years. He collected 158 constitutions of ancient states for empirical study, with the coöperation of members of his group, especially Theophrastus (*Eth. Nicom.*, 1181b, 7, 17; *Pol.*, iv-vi; *Ath. Resp.*). He became the historian of science and philosophy (*Metaph.*, i). He contributed fundamentally to the chronology of literature (*Didascaliae;* see Jaeger, 326), founded poetics and created philology (30:328). He believed that cultures grow and decay like organisms; that there are cycles in the discovery of truth; that the myths and proverbs of pre-literary times embody the discoveries of previous ages (*De Caelo*, 270b, 19; *Metaph.*, 1074b, 11; *Meteor.*, 339b, 28). His ethnological, antiquarian and mythological interests were reflected in the Barbarian Customs (30:328). He believed that man

had lifted himself out of barbarism; but among the abundant evidences of the cultural developments that attested it, he found no corresponding change in the human type. The primeval inhabitants may have been "ordinary or even foolish people," earth-born perhaps, but always men (*Pol.*, 1269a, 5).

Turning now to the *problems of reproduction* and the *development of individual organisms* (*Gen. Animal.*), in which Aristotle was especially interested, it might be said that as an embryologist he was an evolutionist—in the sense of Agassiz, however, not Darwin. A summary of his views in this connection will serve as final words of introduction to a consideration of his views on the development of races.

Aristotle recognized both sexual and non-sexual reproduction; and to this knowledge he added a straightforward belief in spontaneous generation. He knew nothing of cells, so he could have known nothing of the cellular nature of the sex elements. And he knew nothing of modern factual presumptions against abiogenesis. So it was easy for him to see marked resemblances between processes now known to be fundamentally distinct. It was easy for him, in fact, to contemplate sexual reproduction, non-sexual reproduction and spontaneous generation from a single point of view and to reach a unitary conception that presents insuperable difficulties to modern eyes.

Essential to this conception were, first, a material basis for the generated individual, and second, an agency capable of imparting to this material the *motion* displayed in development and the non-material *formal cause* without which none of the potentialities of the developing individual could be finally realized. The material basis was provided by the catamenia (in the human female), by analogous material in other organisms, or by the earth itself. The non-material activating agent was derived from the semen of the male, or from analogous material in other organisms (plants have it "mingled with the female principle within themselves", *G. A.*, 762b, 11), or from earth or air (762b, 14).

Sexual reproduction was characteristic of animals that move about (730b, 33), among whom the sexes were separate. This was in marked contrast with matters in the sessile plants, in which—with the possible exceptions of the fig-tree and the caprifig (715b, 17–25),—the sexes were 'mingled' (731a, 1). The female animal contributed a homogeneous secretion which was in fact a nutritive residue that closely resembled 'primitive matter' (729a, 32; *De Lon. et Brev. Vit.*, 466b, 8). But this she was unable to concoct into semen comparable in powers to that of the male (728a, 19; 765b, 15; 766b, 17), and was, accordingly, in this respect, the inferior sex. The semen of the male was a secretion also (725b, 26), but fully concocted and endowed with the activating agent mentioned in the previous paragraph. This activating agent, however, though usually associated with male semen, was not dependent exclusively on it as a medium. It might pass from the male through the air and be inspired by the female, as when the hen partridge conceives on standing to windward (*sic*) of and within scent of the male *Hist. An.*, 560b, 13). It might even exist, independent of any living organism, in the air or the soil (*G. A.*, 762b, 17). Always, however embodied, it was superior to the material principle supplied by the female. For the formal nature, Aristotle repeatedly insisted, was of greater importance than the material (*P. A.*, 639b-642a).

This was more than two thousand years before the historic experiments that led to the now familiar aphorism: *Omne vivum*

*ex vivo*. There were no such crucial facts to compel Aristotle's attention. For him, the offspring was not of necessity an organized fragment of the parent. He knew very well that plants complete themselves from slips and cuttings. Such regenerative processes, however, appeared to him only to prolong the life of the individual from which the cutting came, just as buds do (*L. et B. V.*, 467a, 23–31). And he distinguished them for that reason from the reproductive processes typical of animals.

Non-sexual reproduction, however, was not limited to plants. Aristotle was convinced that certain sedentary animals, such as mussels, propagate much as onions do, by lateral budding; and that others emit a 'generative slime' (distinguished from true semen), from which, apparently by a process analogous to fragmentation, many individuals might spring (*G. A.*, 761b, 24ff).

Spontaneous generation produced both plants and animals, the latter as diverse as barnacles, insects, eels, possibly quadrupeds, and even man himself (762b). The organisms appeared in all cases in soil, water, and mixtures of the two, with or without accompanying evidences of putrefaction. The heat necessary for the adequate concoction of such a substrate (supplied in sexual reproduction by the animal itself) was the "heat of the warm season in the environment." Then "the portion of the psychic principle which is either included with it or separated off in the air makes an embryo and puts motion into it" (762b, 17; 762a, 18–26). This was a very different conception from the awakening of material germinal particles with life inherent latent from the beginning of creation—as Anaxagoras imagined, and Augustine, and the pre-formationist Charles Bonnet. Here again, Aristotle showed his relative indifference to the materials out of which the embryo was formed. Whatever their source—in catamenial nutritive residues or in festering soil—they were in every instance nonliving and of secondary importance. The non-material principle that gave motion and form to this inert substrate was the thing of prime concern. There was no organized material continuity, therefore, between parent and offspring in sexual reproduction. What continuity existed was established by imponderable principles; just as continuity was similarly established between a spontaneously generated organism and its inorganic substrate.

Regardless of its source, however, the generative substrate changed profoundly during its development. It began as a homogeneous secretion, perhaps; in any case without preformed organs. To that extent, development was epigenetic (734a, 17–735a). But development was also predetermined, proceeding always according to eternal designs, both individual and specific, toward an end or fulfillment that itself determined every stage of the process (*Phys.*, 202a, 12; *G. A.*, 741b, 7ff). Obviously, however, it did not follow an absolutely invariable course. First of all, the developing individual might be a male, or it might be a female. Then it might resemble one parent, or the other, or a grandparent, or still more remote ancestor. Or it might have no family resemblance at all, though otherwise normal and true to specific type. Or it might depart from the parental type, as when black grapes appear among the customary white (770b, 20). Or it might be defective to the point of frank monstrosity.

All these cases were common knowledge in Aristotle's day, as in ours. By way of explanation, he referred them to accidents of development depending on certain differences between the male and the fe-

male nature. When the former prevailed in reproduction, the offspring was male; when it did not prevail, female (767b, 22). The latter was looked upon as an imperfect male, unable to fulfill the exclusively male function of imparting motion and form to the embryo. Yet the material principle which she furnished reacted to some extent on the male principle much as something which is cut affects the cutting instrument (768b, 17).

Again, the offspring took after the father when the paternal principle in the semen prevailed, and after the mother when it did not. Similarly, resemblances to more remote ancestors were interpreted as results of the success or failure of still other principles pertaining to the male. And, finally, monsters and other defects were but extreme terms in the same series (767b, 10ff). In a sense, all departures from the norm established by the male might be regarded as monsters in greater or less degree. As we have seen, Aristotle did not hesitate to contemplate the female of the species from this frankly unflattering point of view (767b, 6ff).

It should be emphasized that all the variations enumerated above were looked upon by Aristotle as *developmental defects*. In the case of the grape already cited, he expressly denied the possibility of a "change into another nature" (770b, 24). It would appear that for him they were all due to perturbations of developmental patterns—patterns that were eternal in form but sufficiently plastic to accommodate themselves to various resistances in the material substrate without altering their true nature or relinquishing their essential control. This was a natural concession for him to make to the familiar facts of every day experience with which every naturalist is confronted. At the same time, he did not compromise his lifelong belief in the primacy of immutable forms that governed not only his views on reproduction and development but his more rigidly architectural conception of the celestial mechanism.

Up to this point in our consideration of Aristotle as a possible contributor to the evolution idea, he has appeared as a *cosmologist*, fashioning his universe according to fixed, eternal plan; as a *geographer* and *historian*, recognizing changes in the topography of the earth and in the conditions and cultures of men, but without changes in the nature of man himself; as an *embryologist*, describing the gradual differentiation of individuals from simple beginnings into fully developed adults, but silent on the possibility of similar differentiation from humble beginnings of the races to which they belong. This last phrase suggests the final focus of our attention. For notwithstanding the contrary presumption that has been accumulating in the foregoing paragraphs, did he after all somewhere express, as Osborn and those who followed him have thought, "the modern conception of the Evolution of life, from a primordial, soft mass of living matter to the most perfect forms?"

Aristotle "was attracted to natural history" says Osborn (43),

> "by his boyhood life upon the seashore, and the main parts of his ideas upon Evolution were evidently drawn from his own observations upon the gradations between marine plants and the lower and higher forms of marine animals. He was the first to conceive of a genetic series, and his conception of a single chain of evolution from the polyps to man was never fully replaced until the beginning of this century" (p. 44).

The last sentence is of especial importance and has already received some attention in the second section of this paper. It appears to be based on two passages from Aristotle. The first, from the *Historia Animalium*, (588b, 4ff) reads, in the translation of D'Arcy Thompson (19) as follows:

"Nature proceeds little by little from things lifeless to animal life in such a way that it is impossible to determine the exact line of demarcation, nor on which side thereof an intermediate form should lie. Thus, next after lifeless things in the upward scale comes the plant, and of plants one will differ from another as to its amount of apparent vitality; and, in a word, the whole genus of plants, whilst it is devoid of life as compared with an animal, is endowed with life as compared with other corporeal identities. Indeed, as we just remarked, there is observed in plants a continuous scale of ascent toward the animal. So, in the sea, there are certain objects concerning which one would be at a loss to determine whether they be animal or vegetable. For instance, certain of these objects are fairly rooted, and in several cases perish if detached; thus the pinna is rooted to a particular spot, and the solen (or razor shell) cannot survive withdrawal from its burrow. Indeed, the entire genus of testaceans have a resemblance to vegetables, if they be contrasted with such animals as are capable of progression.

"In regard to sensibility, some animals give no indication whatsoever of it, whilst others indicate it but indistinctly. Further, the substance of some of these intermediate creatures is fleshlike, as is the case with the so-called tethya (or ascidians) and the acalephae (or sea-anemones); but the sponge is in every respect like a vegetable. And so throughout the entire animal scale there is a graduated differentiation in amount of vitality and in capacity for motion."

The second passage is from *De Partibus Animalium* (681a, 10), here quoted in the translation of W. Ogle (13):

"The Ascidians differ but slightly from plants, and yet have more of an animal nature than the sponges, which are virtually plants and nothing more. For nature passes from lifeless objects to animals in such unbroken sequence, interposing between them beings which live and yet are not animals, that scarcely any difference seems to exist between two neighboring groups owing to their close proximity."

In these passages Aristotle paints with a broad brush a panorama of the contemporary nature with which he was personally acquainted. His thoughts are confined to the present. The transitions that he recognizes in nature as he knows it, with its less than six hundred kinds of animals, are very crude compared with the subtler transitions that Linnaeus and Cuvier and Agassiz recognized long afterward in the thousands of species that crowded their study tables. Cuvier, the founder of palaeontology, and Agassiz, who also gave careful attention to both past and present, constructed far more complete series of far more closely similar forms. Agassiz even based his distinctive classification of animals on his observation of striking resemblances between the adult stages of extinct species and embryonic stages of species now living. But the implication of these facts, so plain to Darwin, Agassiz expressly repudiated in the very last work that came from his pen. He died outside the fold of Darwinian evolutionists.

This being true of Agassiz, especial caution would seem to be indicated in interpreting the passages quoted above from Aristotle. For Aristotle was totally ignorant of palaeontology, and knew nothing of resemblances between extinct and living forms. Nature passes from lifeless objects to animals in unbroken sequence without the least suggestion of the past. No mention is made of the derivation of the organisms of which his scale is built—not a word of a genetic, or, as Newman prefers, a *phylogenetic* series. Positive evidence for the view that Aristotle was an evolutionist—in the Darwinian sense—appears, therefore, to be lacking. Which is not surprising, when it is remembered that to Aristotle species were immutable.

This crucial fact, however, has not always been remembered by commentators. It is not mentioned by Osborn, or Clodd, or Locy, or Newman, or Nordenskiöld, all of whom reckon Aristotle as a racial evolutionist, or Singer, who leans in the same direction. Royce, and Benn, and Mure (39), on the other hand, not only mention the fact, but clearly recognize its implications. Royce has already

been quoted as saying that the Aristotelian "'forms' which are responsible for the evolution of individuals are as eternal as the Platonic ideas. They therefore do not evolve." Benn speaks with equal conviction: "To him [Aristotle] the eternity not only of Matter but also what he called Form,—that is to say, the collection of attributes giving definiteness to natural aggregates, more especially those known as organic species—was an axiomatic certainty. Every type, capable of self-propagation, that could exist at all, had existed, and would continue to exist forever" (p. 292). And Mure: "the infima species—which Aristotle regards as immutable and not evolving—is a single identity. Difference between singular individual specimens serves no purpose; it springs from the irregularity of matter, and is no part of nature's plan" (p. 124).

Aristotle himself says, in *De Generatione Animalium* (14):

> "These, then, are the reasons for the generation of animals. For since it is impossible that such a class of things as animals should be of an eternal nature, therefore that which comes into being is eternal in the only way possible. Now it is impossible for it to be eternal as an individual (though of course the real essence of things is in the individual) —were it such it would be eternal—but it is possible for it as a species. This is why there is always a class of men and animals and plants." (731b, 32ff)

In *De Anima* (17) is a passage to the same effect (415b, 3ff). In *De Generatione et Corruptione* (10) there is a very significant passage which should be quoted in full. After discussing the circular and therefore eternal revolution of the heavens, Aristotle asks:

> "Then why do some things manifestly come-to-be in this cyclical fashion . . . while men and animals do not 'return upon themselves' so that the same individual comes-to-be a second time (for though your coming-to-be presupposes your father's, his coming-to-be does not presuppose yours)? Why, on the contrary, does this coming-to-be seem to constitute a rectilinear sequence?"
>
> "In discussing this new problem, we must begin by inquiring whether all things 'return upon themselves' in a uniform manner; or *numerically* the same, in other sequences it is the same *only in species*. In consequence of this distinction, it is evident that those things, whose 'substance'—that which is undergoing the process—is imperishable, will be numerically, as well as specifically, the same in their recurrence; for the character of the process is determined by the character of that which undergoes it. Those things, on the other hand, whose 'substance' is perishable (not imperishable) must 'return upon themselves' in the sense that what recurs, though specifically the same, is not the same numerically" (338b, 7ff).

These passages have their convincing commentary in the words of Agassiz (2) that follow. Reflecting on the regular succession of individuals in reproduction, he writes: "Whatever minor differences may exist between the products of this succession of generations are all *individual peculiarities*, in no way connected with the essential features of the species, and therefore as transient as the individuals; while the specific characters are forever fixed." In a footnote he adds: "All that is not individual peculiarity is unceasingly and integrally reproduced while all that constitutes individuality, as such, constantly disappears." (p. 11).

It is especially noteworthy that these sentences, expressing Aristotle's thought so clearly, were written to convey the personal convictions, not of Aristotle, but of Agassiz himself.

If, then, species were eternal, and specific characters forever fixed, what was the nature of *hybrids*? Aristotle's consideration of this question is, for our present purpose, quite disappointing. For it is given but incidental treatment, first in a discussion of the essential contributions of the sexes in reproduction (*G. A.*, 738b, 25ff) second, in a discussion of the problem of sterility (746a, 30ff). In the latter, a formal explanation of the sterility of the mule is suggested in which Aristotle

juggles with the idea of species as a logician rather than a biologist, and achieves on an insufficient basis of fact a confessedly empty and barren result (747b, 30–748a, 12). In the former he concludes that the hybrid tends in the course of generations to resemble the female in form, "just as foreign seeds produce plants varying in accordance with the country in which they are sown. For it is the soil that gives to the seeds the material and the body of the plant (738b, 35)." The female parent in the cross provides the soil by which the hybrid is nourished and thus is capable of modifying the direction of differentiation just as in reproduction within the species. There is thus no change in the specific type, apparently; merely a selection of type that is to give its characters to the embryo.

To the problem of *variation* Aristotle gave no consideration except as it concerned individual modifications that altered in no essential degree the specific type. These have already received their share of our attention.

In the field of *heredity*, it is interesting that he distinguished in so many words between congenital and acquired characters (721b, 30). He believed there was satisfactory evidence of the reappearance in the offspring of parental scars and mutilations. The mechanism, however, which Hippocrates (*Airs, Waters, Places*, xiv)—anticipating Darwin's *pangenesis*—had proposed for such cases, he flatly rejected. He opposed it with an array of arguments that it is not necessary to reproduce here, but that were thoughtful and on the whole impressive. He opposed it also as he had long opposed anything suggesting atomistic materialism. "If the parts of the future animal are separated in the semen," he asked, "how do they live?" (*G. A.*, 722b, 3). Instead of a collection of particles coming from all the body, he regarded the semen as a unitary secretion "whose nature is to go *to* all of it." (725a, 23). And though the male "does not give any material at all to the embryo" (764b, 12), it is the semen of the male with which the principle is associated that gives motion and form to the latter.

Thus, in place of the physiological mechanism of Hippocrates, there emerges a conception of inheritance in which nonmaterial forms are the essential elements. This, as we have already seen, he used to elucidate the differentiation of the individual. Nowhere did he suggest for it a similar function in the differentiation of the race. Which again stirs suspicion that such a possibility had not yet occurred to him.

Did Aristotle believe in *xenogenesis*? Certain passages have led some commentators to that conclusion. In *De Generatione Animalium*, Book iii, Chapter 11, Aristotle wrote:

> "and the so-called 'entrails of earth', in which comes into being the body of the eel, have the nature of a scolex.
>
> "Hence one might suppose, in connection with the origin of men and quadrupeds, that if they were really 'earth-born' as some say, they came into being in one of two ways; that either it was by the formation of a scolex at first or else it was out of eggs. For either they must have had in themselves the nutriment for growth (and such a conception is a scolex). . . .
>
> "It is plain then, that, if there really was any such beginning of the generation of all animals, it is reasonable to suppose it to have been one of these two, scolex or egg" (762b, 27–763a, 4).

The scolex is further characterized in an earlier passage: "For pretty much all creatures seem in a certain way to produce a scolex first, since the most imperfect embryo is of such a nature; and in all animals . . . the first embryo grows in size while still undifferentiated into parts; now such is the nature of the scolex" (758a, 32).

The following pertinent passage is from the *Historia Animalium:*

"Eels are derived from the so-called 'earth's guts' that grow spontaneously in mud and humid ground; in fact, eels have at times been seen to emerge out of such earthworms, and on other occasions have been rendered visible when the earthworms were laid open by either scraping or cutting. Such earthworms are found both in the sea and in rivers, especially where there is decayed matter; in the sea in places where sea-weed abounds, and in rivers and marshes near to the edge; for it is near to the water's edge that sun-heat has its chief power and produces putrefaction." (570a, 15ff).

This is the evidence. How shall it be interpreted?

In two footnotes to the first passage quoted, Aristotle's translator, Arthur Platt, remarks:

(1) "These 'entrails of earth' are earthworms almost certainly. A. thinks they are spontaneously generated, and develop into eels." (2) "This is the only passage from which we can gather anything about Aristotle's views on evolution; it appears to have strangely escaped the notice of modern writers on the subject. . . . It is clear that . . . he had no objection to the gradual development of man from some lowly organism, but also that he wisely maintained an attitude of absolute agnosticism on the question". . . .

According to Lones, commenting on the same pasage, Aristotle "seems to have admitted the possibility of generation of men and some quadrupeds from much lower forms of life, for he says that, if generation from the earth did happen, it must have been generation from worms or larvae, or from ova." (p. 82)

But *did* Aristotle actually consider even the possibility of 'the gradual development of man from some lowly organism, as Platt puts it, or the 'generation of men and some quadrupeds from much lower forms of life,' to use the words of Lones?

In our opinion, he did not, for the following reasons: He defined the scolex as an embryonic or larval form common to "pretty much all creatures;" not an adult form. He asserted that if man and quadrupeds were earthborn, they arose, not "perfect in every part, limbed and full grown" but as embryos, that is as scolices; that the 'entrails of earth' (*ges entera*) had the nature of a scolex; that they were spontaneously generated in mud, especially in marginal fresh and salt water; that eels had been seen to emerge from such earthworms and had been found within them. It is reasonable to infer that he associated these worms with caterpillars and other insect larvae from whose bodies in due course adult forms emerged. Possible xenogenesis thus becomes actual metamorphosis, individual, not racial. Thompson (60:149) calls attention to the fact that the larval eel (*Leptocephalus*) is well known to Sicilian fishermen as *Casentula*. This name he derives from *ges entera* through the Sicilian Doric *gas entera*. Could Aristotle himself have seen *Leptocephalus* and recognized its true nature?

The present inquiry has now reached its end. Our conclusion is that Aristotle was not, in fact, either a cosmic or a racial evolutionist. He was familiar with the development of individuals. Why, then, did he not "extend the notion of development to the kind?". Taylor (57:446), who asks this question, himself makes the following reply: "It is the simple fact that, so far as Aristotle or any of his contemporaries could know, there was no evidence for the mutability of organic types."

Is the inclusiveness of this assertion entirely justified? Was evidence perhaps available but for some reason not clearly apprehended? When Lamarck proclaimed transformism, it was not on the basis of evidence that could be said to be demonstrative. Such evidence was lacking when Erasmus Darwin wrote

*Zoonomia* and *The Temple of Nature;* when Spencer read Lyell; when Charles Darwin first caught the idea of descent with modification. It was similarly lacking for Huxley, and Wallace, and Gray. But to them all it was at least sufficiently impressive to be hospitably received. Agassiz repeatedly protested that the demonstration of specific transmutation had not been made; and Baer, even after Agassiz' death, continued similar criticism of the "new Evolution." It can hardly be said that these distinguished naturalists were basing their protests on ignorance. There were no facts available to the Harvard botanist Gray that were not equally accessible to the Harvard zoologist Agassiz. What separated them were not facts but preconceptions. Gray was ready to accept the idea of specific mutability. Agassiz, on the other hand, continued vigorously to reaffirm his old conviction that species were categories of the Creator's thought, divine and immutable.

Similarly, Aristotle had adopted early and retained throughout his life the conception of immutable specific types. Yet the evidence in favor of the fixity of specific types is surely no weightier than the evidence for their mutability. Neither Aristotle nor Agassiz could demonstrate their convictions. Nevertheless, they held them, and looked without enthusiasm on all suggestions to the contrary.

It is idle to inquire whether, had Aristotle held Agassiz' professorship at Harvard, he would have approved *The Origin of Species*. To do so, he would have been obliged to revolutionize his conception of species, accelerating the process by revolutionizing his conception of adaptation as well. For adaptations to Aristotle were manifestations of eternal ends. The significance of vestigial organs appears to have eluded him entirely (*P. A.*, 670b, 13; 689b, 5; *H. A.*, 502b, 24). Discovery, in fact, was but the recognition of such ends in terms of final causes. They were not causes at all in the modern scientific sense. On the contrary, they turned his eyes away from causal mechanisms and postponed for many long sterile centuries the prime discovery of the working hypothesis.

Against the consequences of these retarding influences, so serious for scientific progress under his successors, Aristotle's developing enthusiasm for the concrete particulars of sublunary experience made such advances that the last dozen years of his life in Athens have been more than once distinguished as his *scientific* period.

But was he thereby becoming an evolutionist? Singer may be right in believing that another decade would have removed all doubt of it. At the same time, the reflection obtrudes itself that what Aristotle failed, in his ancient world, to see, Agassiz, in modern days but in a similar universe of ideas, not only clearly saw but with the very last words he penned, expressly disavowed.

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# A CRITIQUE OF PLANT SEROLOGY

## PART I. THE NATURE AND UTILIZATION OF PHYTOSEROLOGICAL PROCEDURES

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[*Editorial Note:* Exigencies of space make it necessary to publish this review in three parts. Parts II and III will appear in the next two numbers following this. The extensive bibliography will follow Part III]

### I. INTRODUCTION

WHILE the subject of the serology of plant products has claimed, to a greater or less degree, the attention of botanists and pharmacologists for about forty years, it is only within the last decade or two that studies in this field have conclusively demonstrated the usefulness of serology in plant research. Particularly through the efforts of Mez and his associates in Germany, of Osborne, Wells, and their colleagues in America, and of a number of independent workers in Europe, the subject of phytoserology has attracted widespread attention in recent years, as is attested by the accompanying bibliography. Up to the present no attempt has been made to collate and unify the rather extensive literature in the field in question. The writer has repeatedly felt the need for such a digest, particularly as the greater part of the literature involved is to be found only in European publications. The present paper is designed as an attempt to meet this need.

In delimiting the scope of such a review it has been felt advisable to restrict the discussion to the serology of plants and plant products exclusive of bacteria. The serology of the phytopathogenic bacteria commands the attention of the phytopathologist, but the growing literature concerning this phase of serology has been dealt with by Link (179). The literature on plant serology as delimited above embraces the techniques and applications of phytoserology in such problems as the following: the identification of plants and plant products; the study of the systematic relationships of plants; the investigation of protein metabolism in plants; the genetic constitution of plant hybrids; the nature and properties of the plant viruses; and the elucidation of the principles of general serology as exemplified in plant materials.

The concepts of serology rest upon the thesis that the introduction into the animal circulatory system of a foreign substance of biological origin, typically proteid (antigen), gives rise to the production in the blood of substances (antibodies) capable of affecting the introduced antigen in a variety of ways, which effects may be demonstrated by a number of types of serological reactions. From the point of view of plant serology these reactions may be briefly characterized as follows:

1. In the *precipitin reaction*, the combination of antigen (precipitinogen) and antibody (precipitin) results in a visible precipitation. "*Conglutination*", so-called, a reaction utilized to considerable extent

by Mez and his associates, may in the form used by Mez be looked upon as a modification of the precipitin reaction, differing in the addition to precipitin and antigen of a third substance, "conglutinin", present in normal cattle serum and believed to increase considerably the sensitivity of the precipitin reaction.

2. In the *anaphylaxis reaction*, the introduction of antigen (anaphylactogen) is followed by the production in the animal of an antibody (anaphylactin) which in combining with the anaphylactogen under appropriate conditions results in the production of more or less violent and spasmodic muscular contractions, of hyperactivity of various glands, of inflammation of tissues, and of other abnormal physiological effects, often resulting in death.

3. In *lysis*, the antibody (lysin) produces a dissolution of the antigen, which in this case is customarily particulate, as for example is true of such antigens as bacteria, blood corpuscles, and in plant serology, plastids, spores, pollen, unicellular algae, fungi, etc. As special modifications of the lytic phenomena may be mentioned the Abderhalden reaction and complement fixation. The *Abderhalden reaction* consists of a detection of the lysis of soluble, proteinaceous, non-particulate antigens, through a chemical analysis of the protein cleavage products formed on the interaction of antigen and lysin. *Complement fixation* rests upon the thesis that a hemolytic antibody requires for its action the presence of a non-specific, heat-labile constituent of normal blood (complement). When an antigen and antibody unite, complement is used up or fixed, and the extent of such fixation of complement, which varies with the extent of serological reactivity of antigen with antibody, is measured by observing whether or not there is sufficient complement unused to permit a second, visible reaction to take place. This second reaction is customarily the lysis of sheep blood-corpuscles by the serum of rabbits immunized with sheep corpuscles, the system being free of complement beyond that which may or may not be left unused by the primary reaction. Under appropriate conditions the lysis of the sheep corpuscles, the observed effect, takes place only in the absence of specific reactivity between the primary antigen and antibody.

4. *Agglutination* of a particulate antigen (agglutinogen) by an immune body (agglutinin) results in a clumping and precipitation of aggregates of antigenic bodies (bacteria, corpuscles, pollen, plastids, unicellular fungi, algae, etc.) which is probably a manifestation of the same antibody as participates in the precipitin reaction, the precipitin or the agglutinin reaction following according as the antigen is in solution or suspension.

5. *Acquired toleration* of toxins and the *neutralization* of toxins and viruses constitutes a fifth expression of immune reaction of animals to plant materials, toxins being rendered innocuous and viruses non-infective by the action of immune neutralizing antibodies.

6. *Phagocytosis*, the ingestion of foreign matter by amoeboid cells of the animal body, is dependent upon the presence in the blood of *opsonins*, which latter may be specifically increased in amount as a result of immunisation with plant products.

The reactions enumerated above have all been applied to the study of plant products, and it has been found that the immunisation of an animal with a plant antigen may result in the appearance of antibodies producing any one or several of the reactions in question, while in addition it has been found that frequently normal, non-immune animal sera will show analogous phenomena in the presence of plant antigens at high concentrations, and conversely that not rarely plant antigens, instead of being precipitated, agglutinated, or dissolved by normal animal serum, affect the normal blood in such a way as to exert analogous actions upon its various constituents.

It is not within the scope of the present paper to go into detail as regards the principles and theoretical aspects of the serological reactions in question, and this phase of the problem will be found adequately presented in Wells' *Chemical Aspects of Immunity*, and Zinsser's *Infection and Resistance*. The details of technique as applied to plant materials, however, as well as such results and theoretical matters as are of chief interest to the botanist, will be dealt with below.

## II. PREPARATION OF PLANT ANTIGENS AND IMMUNE SERA

*Preparation of antigens for immunisation.* Many techniques for the preparation of plant antigens have been utilized, the chief requisites being that antigenic extracts be non-toxic, free from substances producing artefact reactions, and protein-

containing. In much of the work reported up to the present, solutions of the proteins of pulverized seeds have been used, the solvent most frequently being .85 per cent NaCl. Because of aspecific artefact reactions which may result from the non-protein constituents of seed powders, it has become customary to precede protein extraction by a thorough preëxtraction with ether, petrol-ether, benzol, or acetone, followed by a second preextraction with concentrated ethyl alcohol. More will be said subsequently of the value and hazard of the alcohol preextraction, since this is one of the controversial differences between the Königsberg and Berlin schools of plant sero-systematics, but it may be said that in general the alcohol preëxtraction is to be recommended provided that it is not continued so long as to result in a possible denaturation of the antigen. The plant proteins as a rule are more resistant to both heat and alcohol than are the animal proteins, and the danger of denaturation is not as great as is often believed (263). It should be mentioned, however, that certain types of plant protein, the prolamines (e.g. zein, gliadin, hordein), are soluble in strong alcohol. Such proteins are to be found in the seeds of nearly all grains, and doubtless elsewhere in the plant kingdom, and in the preëxtraction of plant tissues with alcohol, allowance must be made for the possible loss of such proteins.

Preëxtraction, besides removing substances which may cause artefact reactions, also has the advantage of removing many substances which are of no value in the serological work, and which are disadvantageous in their toxic effects on the experimental animals or in the fact that they may be largely responsible for difficulty in rendering plant solutions sufficiently clear for serological testing (e.g. gums and resins). Thus non-preextracted solutions are frequently slimy, thick, and opalescent, and this is often obviated by preëxtraction. The alkaloids of plants (e.g. nicotin, atropin, strychnin) are frequently responsible for the death of animals during immunisation, and these may be largely or entirely removed by alcohol preëxtraction. Mez recommends the addition of a small amount of tartaric acid to the alcohol used in preextraction (221, 390) as this mixture will dissolve certain toxic alkaloids, e.g. those of the Coniferae, which are not dissolved in pure alcohol. Preëxtraction is usually continued for several hours or even for one or more days, in an extraction apparatus. Acetone has been used very rarely as a preëxtractant (41) but might well be considered, as it is a solvent of resins and gums which frequently give trouble in antigen preparation.

Having considered preëxtraction one is next confronted by the problem of protein extraction. Instead of the more commonly used isotonic saline, either water or weak alkali may be used. Both the Königsberg and Berlin schools have turned to alkali in cases where protein tests show little chemically demonstrable protein in saline extracts. 0.1 to 1.0 per cent NaOH is the usual concentration. At first the Königsberg workers were of the opinion that there was no serological difference between the NaCl and NaOH extracts of the same plant materials (220, 278, 192, 304), but this has been contradicted by the Berlin workers (96, 7) and more recently by Guttmann at Königsberg (117). Subsequent neutralization with acetic acid is recommended if higher concentrations of alkali are used, but the precipitate resulting from neutralization should not be filtered but should be injected in suspension, since this precipitate contains considerable quantities of native protein. A

small amount of phenol may be added to the extractant to check decomposition (390, 292, and others) without affecting the serological results. Water as a solvent is not generally recommended. Phosphate buffers have been used by Gohlke (104, .85 per cent $Na_2PO_4$)and the writer (47, $KH_2PO_4$ + $K_2HPO_4$, M/10 at pH 6–8) with success. The time required for satisfactory extraction depends upon both material and temperature, most workers extracting from 1/4–2 hours at 37°C., or from 8–24 hours on ice. In certain types of experimentation strong salt solutions have been used as extractants (84), but such a procedure is not recommended as a customary technique.

The concentration of antigen for immunisation varies according to the material in hand, but most workers have used from 5–20 parts of dry tissue per 100 cc. of extractant. The antigens for immunisation should be as concentrated as possible when one is dealing with plant extracts, in which the concentration of soluble protein is often very low. Such antigens need not be clear, but it is desirable to filter or centrifuge them to avoid animal injury or even death by embolism if the venous route is used in immunisation. The materials to be extracted are rarely sterile, although a thorough preëxtraction serves to sterilize the tissue pulp. While many authors lay particular stress on the desirability of sterility of extracts, in the writer's experience this has seemed relatively unimportant provided due attention was paid to chemical cleanliness of apparatus. In spite of the fact that non-sterile extracts have been used in the immunisation of hundreds of laboratory animals, no case of animal loss has been traceable to lack of sterility of the immunisation antigen.

In the maceration of tissues, simple grinding in clean mortars is usually sufficient. The addition of pumice or other abrasive is not necessary except in cases where maceration is difficult, as with delicate fungi (221, 390, 180), and elsewhere the use of abrasives is to be avoided because of the danger that antigen may be adsorbed to the abrasive and thus prevented from becoming dissolved in the extractant.

The techniques described above apply typically to dried seed powders and all other dried plant parts. Alternative satisfactory procedures involve the use of fresh tissues. Expressed saps are in some cases more satisfactory vehicles of immunisation antigens than saline extracts of fresh tissues, and such saps may often be used without preëxtraction in the production of specific sera, and as test antigens. If artefact-producing substances are present, they can frequently be removed by dialysis. Such saps are usually of pH 5–6, but neutralization has not been found necessary. Expressed saps have been used successfully by Birkeland (29), Franz (96), Magnus and Friedenthal (188, 189, 190), and the writer (47, 50). Maschmeier in addition (196) used expressed sap dried in thin layers, pulverized, and later preëxtracted and extracted in saline.

Similarly fresh tissues may be macerated, preëxtracted or not, and then extracted. Ziegenspeck in Königsberg has recommended such a procedure for plant serology (390), and Bürger in Berlin also finds it suitable. The students of the serology of the plant viruses have in general used saline extracts of fresh vegetative tissues, and other workers report success with such a technique (196, 26, 84, 69). The choice of sap, extract of dried tissues, or extract of fresh tissues will depend on the material available, but all may be used with success.

When dealing with small, particulate

antigen-containing bodies, e.g. unicellular algae and fungi, pollen, plastids, etc., it is entirely feasible to suspend the unmacerated bodies in physiological saline or olive oil, and then inject. The latter procedure has recently been adopted by the Königsberg workers (169), and there have been numerous reports of success using for immunisation saline or oil suspensions of macerated vegetative tissues (225, 56, 105, 26, 180), of algae (300, 188), and of yeasts and fungus spores (167, 207, 178, 64, 323, 34, 198).

A common type of antigen in the serology of bacteria is the filtrate of the substrate on which the bacteria are grown. Such an antigen has little place in most phytoserology, but Matsumoto (197, 198) obtained good precipitin results using filtrates of *Aspergillus* cultures, when antigens prepared from dried mycelium and and spores were unsuitable.

Purified or altered protein preparations have been used as antigens by numerous workers in plant serology, the techniques of antigen preparation being too varied and specialized to discuss in detail, particularly as a separate section below will deal to a greater extent with this subject. Much of the work dates back to the classic studies on the purification of plant proteins by Osborne (263) and the subsequent immunological studies of such purified proteins by Osborne and Wells and their colleagues, the purification being accomplished by taking advantage of such differential properties of the proteins as their solubilities in water, salt solutions, acids, alkalis, and alcohol. Such proteins frequently but not invariably show an extremely high degree of antigenicity as judged by various tests. Modified plant proteins, such as iodized, diazotized, and peptized proteins, have also found a place as antigens, but a discussion of these specialized types of antigens is better taken up at the time when the results of plant-serological tests are considered.

Similarly, various non-protein plant substances have been used as "antigens" in attempted immunisation of animals. Chief among these are chlorophyll and xanthophyll, starches, glucosides, and other carbohydrates, plant lipoids, and alkaloids. The preparation of such "antigens" and the result of their serological study will be considered subsequently.

The question of whether preserved plant materials may be used as antigens is of practical importance. The Königsberg workers have found that, while plant materials preserved in $H_2SO_4$, $HgCl_2$, and formalin are unsuitable for serology, materials preserved in alcohol are entirely suitable, and dried herbarium specimens which have been so preserved for many decades still give tests comparable to those given by fresh material of the same species (217, 210, 211).

Whether, on the other hand, various tissues of the same plant may be used indiscriminately in sero-systematic work, is a question which has been debated by the phytoserologists.

The Königsberg school maintains that all tissues of a plant react similarly (217, 220, 211, 117, 390, 214, 380), a position which accords with Mez' view that the serologically active component of plant extracts is the nucleo-protein or ideoplasm which is presumably identical in all cells (213, 214). Likewise, Magnus and Friedenthal (190, 191) were unable to detect any significant serological difference between seed, pollen, and root antigens of rye, although from their tables this identity is not as well indicated as one might expect. The Berlin workers, on the other hand, find that leaf and seed extracts of the same plant behave quite differently serologically (209, 39, 38), and this finding is confirmed by Moritz and vom Berg in Kiel (235, 24), the Berlin work resting on the precipitin test, the Kiel work on the delicate uterine-strip (Schultz-Dale) anaphylactic test. Dunbar (69) has reported complement fixation tests in which rye pollen, rye grain, and rye leaf sera were each highly specific for its own tissue. Azuma (12) found serological (anaphylaxis) differences between seeds

and seedlings of grains. Kubeš (165) in complement fixation tests had no trouble in differentiating seed, leaf, and root of the poppy. Wodehouse (382) believed that pollen and seed proteins of the same plant are probably anaphylactically distinct. Finally, Lieske (176) reports that chlorophyll-containing and chlorophyll-free cultures of the same species of green alga are to be distinguished serologically (agglutination).

On the whole, the evidence seems to indicate that various tissues of the same plant may show serological differences. This is particularly in line with the finding of the investigators of purified seed proteins that such storage proteins give definite chemically-specific but not species-specific serological tests, and since such storage proteins are lacking in other parts of the plant than storage tissues, it it follows that the storage tissues will presumably react serologically differently from the vegetative tissues. This possibility must accordingly be borne in mind in the selection of plant tissues for the preparation of antigens. Wartenberg (359) has distinguished between species-specific and organ-specific proteins. The latter, exemplified in animals by eye-lens protein, are relatively uncommon in plants, the majority of the organs of a plant being fairly uniform serologically, but the possibility of the occurrence of organ-specific proteins makes it advisable to limit single experiments to a single type of organ. The problem of antigenic variation in a given individual at different stages of development has never been adequately explored. Possibly there may occur a recapitulation of protein structure during ontogeny, comparable to morphological recapitulation.

For animal injection the toxicity of plant tissues must be considered. Many plant tissues are very highly toxic to animals, e.g., those of the Coniferae, especially the Taxaceae and Gnetaceae, *Rhododendron*, *Phytolacca*, *Melandryum*, *Pisum*, *Aesculus*, *Dipsacus*, *Allium*, *Iris*, *Canna*, *Thea*, etc. (221). The toxins of such plants may render successful immunisation very difficult. Proper preëxtraction will aid in the removal of toxic substances. In addition, or as an alternative, dialysis through collodion (Du Pont "cellophane") rapidly removes the highly toxic alkaloids of such plants as tobacco, potato, and tomato, leaving the proteins behind. By dialyzing against a buffered salt solution, one may at the same time stabilize the salt concentration and the pH of the antigenic solutions. Frequently when plant serologists have faced the difficulty of toxic extracts, the solution has been to select another related but toxin-free species, and in a study of plant relationships such a practice might sometimes suffice.

Nearly all workers in plant serology have emphasized the desirability of some method of measurement of the protein in antigenic extracts. Of the numerous techniques which have been used, those chiefly concerned are the Esbach test, Kjeldahl determinations, and the hot nitric acid test or "Kochprobe," although the trichloracetic acid test, the biuret reaction, precipitation with ammonium sulphate, the Millon test, the nitro-prussic reaction, and the sulphosalicylic acid test have all been mentioned in connection with the preparation of plant antigens. In the case of the Esbach test, Esbach reagent (picric acid:citric acid:$H_2O$ = 1:2:100) is added to the extract, and the protein precipitate resulting is either estimated as to amount or measured in an albumimeter tube. The Esbach test has been more frequently employed in the Königsberg school, the nitric acid test in Berlin. Both schools recognize the inadequacies of such tests, however. The difficulty lies in the fact that not only is there no theoretical reason for

expecting that immunologically active protein and chemically demonstrable protein should be correlated, but actually numerous investigators (217, 211, 228, 333, 390, 245, 38, 105) have found that good serological reactions may be obtained with extracts giving poor or negative protein tests (usually Esbach), and that some chemically active proteins are poorly reactive serologically. Furthermore none of these protein tests is specific for protein in the sense that it reacts with all protein and only with protein. In spite of these facts, many investigators have not only felt that it is necessary to keep a record of such tests, but that in addition all extracts of an experimental series should be diluted to an extent where all give the same test with the protein indicator used (129, 211, 17, 98, 385, 8, 196, 36). If there are motives for so doing, there are motives that appear at least equally strong for holding constant not the protein test but the ratio of plant tissue to amount of solvent, and it may very well be that in some cases serological results have been badly distorted by the adjustment of all extracts to the same Esbach test.

Finally, a word should be said as to the preservation of antigenic extracts. When plant powders are used, a most satisfactory procedure is to seal the powders in brown bottles which are kept in the cold, solutions being made whenever required. Such powders keep indefinitely without deterioration. Extracts and saps may be preserved on ice with or without the addition of phenol (1 per cent) for considerable lengths of time (a few weeks to a year or more), and in some cases the preservation of antigenic extracts and saps frozen solid may be continued a year or more without appreciable loss of reactivity. The drying of extracts on paper with subsequent re-solution in saline has been suggested as a means for antigen preservation, but the method has never come into general use, and the theoretical advantage of accuracy in measurement seems to be outweighed by a number of practical disadvantages.

*Immunisation of animals and preparation of sera.* For the preparation of sera immune to plant antigens, any of a variety of animals may be used, although in the greater part of the work thus far reported rabbits have been employed. Dogs have been used successfully for the production of immune sera by Cao (42), Corpaci (61), and de Angelis (5), goats by Dunbar (68) and Ehrlich (73), horses by Dunbar (68) and Müller (243), and guinea pigs by Corpaci (61) and others. For anaphylactic sensitization the guinea pig is almost universally used. In preparing immune sera, rabbits weighing from 1500 to 2000 gm. are preferable, and a period of quarantine before inoculation is desirable in order to minimize the danger of loss from coccidiosis.

The question of the feeding of animals for immunisation is an important one theoretically, because of the possibility of the production of immune bodies due to immunisation *per os*. It is well known that animals can be immunized by feeding certain plant toxins to a point where many normal lethal doses may be eaten with impunity. Thus various animals can be immunized to ricin (243, 72, 135, 226) to a point where they may resist as much as 5000 normal lethal doses (62), and rabbits can similarly be immunized to the *Amanita* toxin, phallin (86), yet while it is comparatively easy to produce specific precipitins by inoculating animals with ricin, no specific precipitins are to be found in the blood of animals which have been fed large doses of ricin (226). Similarly rabbits and guinea pigs fed exclusively on rape, radish, lettuce, or cab-

bage yielded no precipitins in their sera for extracts of these plants, although cutaneous immunisation produced such precipitins (128). Again Wendelstadt and Fellmer found that exclusive feeding of potato, pea, or corn produced no precipitating, complement fixing, or anaphylactic antibodies for proteins of these plants (370). Worseck also had similar results (383). Hiki (125) claimed that the feeding of guinea pigs and dogs with bean protein and egg white gave rise to sera which contained precipitins and anaphylactic antibodies for these proteins, but the titers were low and the work has not been confirmed. It would thus seem evident that there is comparatively little danger in immunisation due to the appearance of antibodies produced by ingested food, although in the serological testing of articles involved in the diet of experimental animals it would be well to keep such a possibility in mind. One need not feel compelled to use carnivorous animals in the serological study of plant proteins.

As regards route of immunisation, intraperitoneal and intravenous inoculations are in most general use, particularly the former. Intravenous inoculations have been claimed to produce high-titer sera rapidly, but in the experience of the majority of workers the intraperitoneal inoculations are fully as satisfactory and avoid certain difficulties and dangers of intravenous inoculation (e.g. animal loss through embolism, anaphylactic shock, or extract toxicity). Extracts for peritoneal injection need not be free of particles, and sterility is of less concern with this type of inoculation. A combination of one or two intravenous inoculations followed by several intraperitoneal inoculations is frequently used to advantage. Intracutaneous inoculations have been frequently used, particularly in the earlier work on plant serology, but while it is often possible to produce immune sera by this route, it is not recommended for general use as immunisation is slow, the titers are not as high as by the venous and peritoneal routes, and in plant extracts there are frequently present substances which irritate to such an extent as to bring about the formation of large abscesses at the site of cutaneous inoculation. Immunisation *per os* may be used for the production of an immune state to many plant toxins, but, as has been seen above, this is not a satisfactory route for the production of immune sera or an anaphylactic condition.

The schedule of inoculation, i.e. the amount of extract inoculated, and the frequency and number of inoculations, will vary with the concentration of the inoculum. Since antigenic plant extracts are for the most part relatively poor in protein, one usually uses a greater amount of inoculum and longer period of inoculation than is customary with the more highly concentrated animal antigens. Using extracts of whole plant tissues it has been the practice to inoculate rabbits with from 25–100 cc. of extract in 6–10 injections at intervals of 3–4 days. Rabbits show great individual differences in susceptibility to immunisation, and with some individuals it is extremely difficult or impossible to obtain high titer immune sera. Over-injection has been avoided by the Königsberg school because of a possible danger of loss in titer from too long continued inoculation. Longer intervals than 4–5 days are to be avoided because of the danger of anaphylactic shock after a week or more following injection. From 5–10 cc. of inoculum may be injected peritoneally and from 2–5 cc. intravenously, as a general rule, without undue ill effect on the experimental animal.

In the writer's experience the following technique has produced excellent precipitating and neutralizing sera with little animal loss (50): non-preëxtracted expressed sap, inoculated intraperitoneally in 5 cc. doses except for the first dose of 2 cc. with 5–8 injections at 3–4 day intervals, followed by a rest period of 9–10 days before bleeding. The animals thus inoculated in general showed a slight loss in weight after the first injection, which was soon regained, and at the time of the last injection most animals showed a gain in weight over the immunisation period of 100 to 500 gm. Using purified protein solutions the dosage and length of the period of immunisation may be considerably decreased, and if chemically pure plant protein is used a single injection of a few milligrams may suffice to produce high-titered precipitating serum 10–20 days after inoculation.

It is quite customary among phytoserologists to make periodic blood probes during the course of immunisation, as for example after the 3rd, 6th, and 9th inoculations. The blood probe is made 4–6 days after the injection, usually from a sample of about 5–10 cc. of blood drawn from the marginal ear vein or heart of the rabbit. This blood is allowed to coagulate, the serum is centrifuged clear, and serological tests are performed to determine the titer of the serum. Blood probes are desirable when one is in considerable doubt as to the length of time necessary for adequate immunisation, or when, as in some types of sero-systematic work, a low-titer serum is desired.

Preliminary to the first inoculation it is desirable to take a sample of 10–15 cc. of blood from each rabbit, which will serve in later work as a normal serum control for the serological tests with the immune serum from the same animal. Such a procedure is preferable to the rather common practice of taking a larger quantity of normal serum from one or several animals which are not used later for immunisation. The normal serum should be preserved in the same manner as the immune serum later obtained, and to insure comparable normal and immune sera, the immune sera may be preserved a few days before using.

After the last inoculation it is customary to wait a week or 10 days before bleeding in order to insure that there is no longer free antigen in the blood. For 24–36 hours before bleeding, the animal should be given no food, as feeding just before bleeding often results in sera which show a tendency to gelatinize or which have a more or less marked opalescence that may render difficult the reading of precipitin or agglutination tests. A similar period without food should precede the taking of normal serum. If the sera are opalescent, heating to 56° for an hour will often eliminate this without destroying specific reactivity.

In bleeding it is preferable as a rule to take all the blood possible from an animal rather than to take sub-lethal samples at intervals after immunisation. This insures the production of 30–40 cc. of comparable immune serum from a rabbit, and there is less danger of change in titer from preservation *in vitro* than from preservation *in vivo*. Sterile blood may be obtained either by bleeding from the carotid artery or by dissecting open the breast cavity, cutting the aorta, and pipetting out the blood. Both techniques are entirely satisfactory. The experimental animals are anaesthetized, as with ether, chloroform, etc., preliminary to bleeding. The drawn blood is permitted to coagulate for an hour or two at room temperature or over night on ice, the clot is loosened from the sides of the tube, and the serum is centrifuged twice.

Sera may be preserved in a variety of ways, as by the use of glycerine, phenol, thymol, toluol, or chloroform, or by storing at refrigerator temperatures, freezing solid, drying on paper or drying to powder with subsequent re-solution in physiological saline, or by asepsis or bacterial filtration, or a combination of these measures. All the techniques mentioned have been used in phytoserology, and all are useful with the exception of the method of drying which has not given entirely satisfactory results.

In preparing animals, chiefly guinea pigs, for anaphylactic tests it is customary to give a single intraperitoneal sensitizing dose followed by the test for anaphylaxis approximately 18–24 days later. If, instead of inoculating a plant antigen solution for sensitization, an intraperitoneal injection be made of 2–3 cc. of anti-plant precipitating serum, the animal becomes passively sensitized and will react specifically to the antigen used in preparing the serum. The test for anaphylaxis in this case may be made 24 hours after the intraperitoneal inoculation of the immune serum or 8 hours after if the inoculation of serum is intravenous.

A certain amount of animal loss usually accompanies immunisation with plant antigens. With rabbits, coccidiosis is a frequent cause of death and can best be guarded against by quarantine before immunisation and subsequent cleanliness in the cages. Much of the animal loss experienced by plant serologists may be avoided by care in handling of animals and use of extracts. The frequent men-

tion of animal loss in the literature is largely due to embolism as a result of air bubbles or solid particles in antigenic solutions injected directly into the circulatory system, failure to permit animals to recover from previous inoculation before reinoculation due to too heavy dosages or too short intervals of inoculation, and toxicity of plant antigens. Anaphylactic shock is rarely responsible for animal loss in immunisation. It is advisable to keep weight records of animals undergoing inoculation, regulating the dosage according to the gain or loss in weight. In case of serious loss of weight after an inoculation it may be necessary to resort to an emergency bleeding even though the period of immunisation is not complete, or better to inject a very small quantity of antigen, sufficient to desensitize enough to prevent anaphylactic manifestations in the subsequent inoculation, but not enough to cause a serious toxic reaction in the animal. In the case of suspected toxicity of the plant material, it is advisable to prëextract thoroughly or to dialyze the extracts before inoculation.

A section on the preparation of sera for plant serology would be incomplete without mention of the artificial sera or "Künstsera" devised by Mez (221, 390, 216, 225, 214) and used with apparent success by a number of his associates in the preparation of the Königsberg serosystematic genealogical tree of plants. The preparation of Künstsera rests upon Mez' assumption that "antitoxins are no more than de-toxified toxins" (Büchner), and that antibodies in general consist of the antigenic molecules modified by ferments present in normal blood serum. If this be true, then it might be possible to prepare antibodies by judiciously mixing and incubating antigens and normal serum. Mez' technique for the artificial preparation of immune sera is as follows (390):

> Three cc. of plant extract (1:200 solution of tissue powder) is mixed with 1 cc. fresh beef serum and allowed to stand 8 days at 35°C. with daily agitation. This mixture is then diluted with 20 cc. of phenolized (.5 per cent) physiological saline solution and centrifuged clear. Three cc. of fresh beef serum (complement) is added and enough NaCl solution to make a total of 30 cc. Such artificial sera are then tested for precipitin titer against the antigens employed in preparation and have been found to have titers of 1:3200 or 1:6400, and are claimed to be perfectly comparable to "natural" sera, or even more specific (221). To increase the titers of such sera one substitutes for the beef serum the serum of a rabbit which has been injected with a foreign, heterologous protein (e.g. milk) 12 hours previous to bleeding.

The Künstsera are held to be preferable to natural sera in that one avoids animal loss through toxicity of antigens, and that the same beef serum may be used to prepare many sera, a step toward homogeneity in serological materials. A patent has been applied for. A number of observed phenomena in serological (precipitin) routine have been explained by Mez in terms of the Künstsera hypothesis, such as the clouding of mixtures of normal serum and plant antigens after long-continued incubation, and the fact that in the preparation of Künstsera there first appears a precipitate in the incubated tubes (the action of the first, low-concentration artificial antibodies on the plant protein) which is later redissolved. A year previous to the publication of the first accounts of the Künstsera by Mez, Jena in Germany (139) obtained a patent for a comparable process for the preparation of "substances similar to immune bodies" (e.g. diphtheria toxin + pancreatin + inactive blood serum + 24 hrs. at 37° → antitoxin). Even as early as 1915 Ostromyslensky in Russia (265) had published enthusiastic accounts of the preparation of antitoxins *in vitro* by mixing serum globulin with toxin (botulism,

diphtheria, staphylotoxin), incubating, and then breaking down the new compound into antitoxin and free toxin by means of HCl. These antitoxins were claimed by Ostromyslensky to be indistinguishable in neutralizing and therapeutic effects from natural antitoxins. Kostoff (156) has mentioned, without reference, studies purporting to have obtained similar results in the production of antibodies *in vitro* by Kabilek (1927), Sdrawosmislow (1927), Baschkirtzev (1929), and Krishanovsky (1929).

The great success reported by Mez with artificial sera led to a number of attempts to confirm the work in other laboratories. Of the phytoserologists, Moritz (232), 230), Boom (36), Grijns (116), Nahmmacher (245), and Sasse (308), and among the animal serologists von Eisler (75) have all attempted to repeat Mez' work following his techniques in detail and with variations, and none has had much success in obtaining specific sera by this method. Sasse obtained 8 artificial sera in 26 trials, but the titers were low and the sera showed little safety for the study of plant relationships. Nahmmacher's results were similar. The sera obtained by Boom, Grijns, and von Eisler were entirely worthless serologically. Even Ankermann in Mez' laboratory at Königsberg, when speaking of using artificial sera in connection with serological work on the Monocotyledons, states that a number of the artificial sera were unsatisfactory. These attempts at confirmation appear on the whole to be both thorough and impartial. In particular this may be said of the work of Boom, who substantiates Mez in most particulars apart from the matter of Künstsera. Such being the case, it is extremely difficult to understand why this work is claimed to have such success in Königsberg. Perhaps, as Boom suggests, Mez and Ziegenspeck have not described the method adequately. Perhaps there is an element of truth underlying the conflicting reports, obscured thus far by inadequacies in technique. At least, however, one does not feel in a position at the present moment to recommend the use of Künstsera in plant serology as a technique for general application.

*Preparation of antigens for testing.* In general the same remarks as apply to the preparation of antigens for immunisation apply to the preparation of test antigens. Certain additional precautions must be taken, however, such as clarification, exclusion of artefact reactions, and adjustment to appropriate concentrations.

In the anaphylaxis and neutralization reactions it is not necessary to have clear antigens. In the other serological reactions with plant antigens, and above all in the case of the precipitin reaction, it is necessary to have extracts which are crystal-clear and which contain as little coloring matter as possible. This, with plant antigens is often a requirement difficult of fulfillment, and numerous techniques have been employed for clarification of extracts. Filtration through paper, particularly the hard papers, will sometimes suffice, and will often serve as a preliminary step. Where extracts are very turbid this paper filtration may be preceded by filtration through a coarser paper or gauze. With slimy extracts it is sometimes desirable to prepare a large excess of extract and to save and re-filter only the first portion passing through the filter. In addition to the paper, in refractory cases, use may be made of filters of asbestos, kieselguhr, or finely divided glass, using suction or pressure to force the liquid through. At times centrifuging will suffice for clarification, at other times this may be employed as a useful adjunct to other methods. It has been

suggested that in difficult cases one may adsorb the colloidal matter from cloudy extracts by means of animal charcoal or other finely divided adsorbant, but this is not recommended as a general practice because of the loss of protein necessarily involved. In fact, with any type of filtration one must count on a certain protein loss. It may be repeated here that thorough preëxtraction will aid in later clarification. An excellent technique for some proteins is the practice of alternately freezing and thawing several several times. The precipitate resulting from this procedure is easily thrown down by centrifuging and there is often no serious loss in concentration or volume of antigen. Another practice consists in discarding refractory extracts and the choice of closely related plant species for testing. Extracts of dried tissue powders as a rule are much more easily cleared than extracts or saps of fresh tissues.

In immunisation it is customary to use antigens in as concentrated form as possible. In testing, on the other hand, it is better to dilute considerably. The students of plant serodiagnosis ordinarily use a concentration of 1:200 of plant tissue powder in physiological saline solution. Expressed sap, properly cleared, may be used without further dilution, and usually represents a concentration of about 1:50 to 1:100 in terms of dry weight. Saline or aqueous extracts of fresh tissues are proportionately weaker. Since with tissue powders concentrations are expressed in terms of dry weight, while with saps these are expressed in terms of the volume of the total sap, a titer of 1:100 with expressed sap is of the same order as 1:10,000 with powder extract or 1:100,000 with purified protein, since the protein constitutes only a small fraction of the dry weight. Since in plant serology the titers customarily observed with powder extracts are of the order of 1:10,000 to 1:50,000, and with saps of 1:100 to 1:500, these titers show an immunological sensitivity of animals toward plant antigens of the same magnitude as the titers usually obtained in serology with bacterial or animal antigens.

Normal serum frequently precipitates in the presence of plant antigens when both are at relatively high concentrations. Although this phenomenon has been attributed to so-called "normal precipitins," this term is better avoided because there is usually little or no evidence that the precipitations are qualitatively of the same type as those caused by immune precipitins, and in a number of cases there is definite evidence that such reactions are due to relatively simple chemical relationships between serum and extract, having nothing to do with antigen-antibody reactions in the accepted sense of the term. Thus, many plant proteins have isoelectric points definitely in the acid range, while serum proteins have alkaline isoelectric points. Under some conditions, the mixture of such will result in precipitation which bears no relationship whatever to specific serum reactions. Such normal serum reactions are also involved in other immunological techniques than the precipitin reaction, and frequently they are sufficiently strong to distort the results of specific reactions and must either be eliminated or taken into consideration in evaluating results.

With respect to the precipitin reaction three techniques have proven successful in eliminating this source of error.

In the first place, and simplest of all, one may dilute antigen or serum or both until no precipitate is observed on mixing and incubating with normal serum. Since the titer of specific serum is necessarily higher and usually much higher than the "normal precipitin" titer, the specific reactions are thus freed of any effect due to normal serum without affecting the utility of the specific reaction. Instead of pre-

liminary tests with all extracts against normal serum, it is customary in much of the phytoserological work to select an arbitrary dilution of antigen (e.g. 1:200 with tissue powder), and when this dilution does not eliminate all normal serum reactions all results are discarded using antigens more concentrated than the least concentration giving a positive test with normal serum. This is the customary technique in the Königsberg school.

A second technique has been utilized by Kōketsu (154, 153) and Kojima (152) in Japan, and consists in the treatment of each antigen with an equal volume of normal serum prior to testing. This antigen-serum mixture is incubated, then centrifuged, and the supernatant fluid, which no longer reacts with normal serum, is used in testing against immune sera.

A third technique as worked out by Hannig and Slatmann at Münster (118, 330, 119) and later used with apparent success by Bürger (39) and Bry (38) at Berlin, consists in the addition of phosphate buffers to each extract. Either a $KH_2PO_4$-$K_2HPO_4$ mixture or a solution of $H_3PO_4$ + KOH is used, preferably with a pH of about 6.5 (e.g. 5 cc. molar $H_3PO_4$ + 6.8 cc. 1.5 molar KOH). In every case tested this eliminated the precipitation with normal serum, while numerous immune serum reactions remained. There is no doubt from the works of these various investigators that normal serum precipitates with plant antigens may be eliminated by the use of such phosphate buffers. On the other hand, Hannig and Slatmann report numerous cases where immune serum was negative with a given antigen until phosphate was added, when a strong reaction appeared. These investigators have reported numerous serological reactions which are not in accordance with the accepted systematics of the plants studied and rather curiously such non-specific reactions involve for the most part combinations of serum and antigen in which the combinations were negative until phosphate was added. Perhaps the phosphate, in addition to eliminating the normal serum precipitates, causes some sort of artefact precipitation, and if so the method is certainly to be avoided until more is known of the effect of phosphate.

Hiki in a study of the serological reactions of bean extracts (125) found that the normal serum reactions customary with ordinary techniques were completely eliminated if the extracts were exactly neutral, while specific immune serum reactions were unaffected. It remains to be seen whether this technique will be of general use, but the evidence of Hiki shows that in one case at least the normal serum reaction has nothing to do with precipitins in the usual sense of the word, since these are relatively independent of pH within a broad range on both sides of neutrality.

Altogether the methods of dilution or absorption with normal serum seem preferable to other methods for the elimination of normal serum reactions. Mez objects to the absorption method on theoretical grounds, namely that the addition to plant extracts of normal serum is likely to result in the production of artificial antibodies *in vitro*, but it would seem that the evidence for such an objection is weak, particularly as an hour or two in the incubating oven is sufficient for the absorption of the normal serum reaction.

### III. PRECIPITATION REACTIONS

The precipitin reaction in various forms has been used more extensively than any other serological test in the study of plant antigens. Briefly, the test consists in the addition to plant extracts of immune serum, the two constituents being either overlayered one upon the other (ring test), or mixed (flocculation test), a period of incubation, and the reading of the precipitate formed. An immune serum may be permitted to act fully and consecutively against two or more antigens (absorption test), the precipitate being removed by centrifuging after each admixture. Such a technique is useful in determining whether two reactions of the same serum are identical or whether the antibodies involved in the precipitation of an heterologous antigen are qualitatively the same as those involved in the homologous test. Ideally the amount of precipitate is greatest when a serum is added to its homologous antigen, less with closely related antigens, and negative with more distantly related ones. The variations of

technique in different laboratories are considerable.

*The flocculation test.* The older, flocculation test has been used to greatest extent in the Königsberg school. As performed in Königsberg the procedure is as follows (210, 211, 390): Crystal-clear antigen solution (1:200) is further diluted with physiological saline 1:400, 1:800, . . . 1:51,200. To 1 cc. of each antigen dilution in a test tube of 10 mm. internal diameter is added .1 cc. of undiluted and untreated immune serum. The tubes are shaken and placed in an incubator at 37°. Readings of the amount of precipitate are taken after 12 hours of such incubation, at which time the flocculation has settled toward the bottoms of the tubes. The antigenic solution may be diluted as above, the serum being held constant, or the serum may be progressively diluted, the antigenic concentration being held constant. As the reaction is independent of complement, the serum used need not be fresh, and it may be inactivated at 57° for 1/2–1 hour without detrimental effect on the reaction.

The Königsberg school uses the following system of controls:

1. 1 cc. of each antigen dilution (or often only the dilution 1:1000) is tested with .1 cc. of normal serum.

2. .1 cc. of each serum is tested with 1 cc. of antigen diluent (saline solution), and 1 cc. of each antigen dilution with .1 cc. of the same.

3. All readings are made "blind", i.e. the observer deals only with numbered tubes the contents of which are unknown to him.

4. All reactions for use in the study of plant relationships are further safeguarded by the following requisites (214): (a) Relationship reactions between two plant antigens must be reciprocally confirmed. (b) The results of precipitin testing must be confirmed by those of the "conglutinin" reaction performed simultaneously on the same material. (c) Plants of undoubted close affinity must show serological affinity and plants undoubtedly not closely related must not show affinity serologically. (d) Plants which are known definitely to be intermediate between others must be intermediate serologically. (e) The summation of all serological relationship reactions must form a logical system. (f) The systematic position of a group must be confirmed by numerous serological reactions proceeding from various reaction centers.

According to the Königsberg requirements, controls 1 and 2 must be negative, 3 must be complied with before an experiment is considered significant, and the qualifications of 4 must be fulfilled before a serological system of plant relationships may be considered valid.

A similar technique, with the important exception that the time of incubation may be greatly shortened with advantage, has been used in many other laboratories. One of the chief differences in technique among different workers involves the method of evaluation of results. In Königsberg it is most common to consider the last tube of a dilution series of antigen which shows a settled flocculation (not a uniform turbidity) as representing the titer of that antigen with the serum used, and the titer is the criterion of strength of reaction. A number of investigators, feeling that the titer alone does not give an adequate measure of the strength of reaction, have laid emphasis on the time required for the reaction to appear or to reach given strength (104, 4, 196, 8, 385, 291, 35, 225), and this alone, or in combination with the titer, is taken as the measure of reactivity of a given antigen-serum combination. Others have felt that neither the time nor the titer are so important as the volume of the precipitate formed at constant concentrations of serum and antigen (383, 260, 46, 347), and this may be fairly accurately observed by centrifuging the mixtures for a constant length of time and at a constant speed in narrow tubes of even diameter, and measuring the height of the precipitate column thus produced (260, 46, 347). Serologists in England frequently prefer to use a method of titration in which the tube of a titration series which shows the strongest

reaction is taken as an index of titer, rather than taking as a measure the most dilute mixture showing a reaction. This method of "optimum concentration" has an advantage when there is danger of a reaction being inhibited by excess of antigen (zone phenomenon), but appears to have no great superiority to the ordinary methods of titration with plant proteins, since the latter are rarely found in excessive concentration. Another method of determination of the strength of reaction is to use simultaneously sera of various titers prepared against the same antigen, for it has been observed (187, 104) that a short period of immunisation produces sera which react only with closely related species, while a longer period of immunisation enlarges the reaction range. Kōketsu (153) used an interesting method of correction of observed results. The amount of protein in the antigens was determined by Esbach reagent, and 1 per cent taken as a standard. The observed titer was then doubled if the protein test was half as great as the standard, and halved if twice as great, etc. Secondly, this corrected value was again corrected by a factor which would make all sera show the same titer against their homologous antigens. Meyer (209) used a simpler but similar type of calculation.

That so many methods have been devised for measurement of strength of precipitin reaction is indicative of the recognized inadequacy of any one. If a reaction is positive, many factors will determine the intensity of precipitation, and it may be that much of the precipitate consists of non-specific substances apart from antigen and antibody. In our present state of comparative ignorance of the mechanism of the precipitin reaction it seems rather futile to argue as to the relative value of the various techniques outlined. If, for example, any two reactions are so nearly alike that the results obtained by using time of appearance as a criterion differ from those using titer or amount of precipitate as a criterion, then one would not be justified in considering them significantly different. Particularly when dealing with such complex and unknown solutions as extracts of whole plant tissues one has little justification for reporting as significant any results which are not so well marked that significant differences are shown by all three criteria, time, titer, and amount of precipitate. As to the method of correction of titers used by Kōketsu, this must be considered as highly liable to error. We have already seen that one of his factors of correction depends on the Esbach test which may bear little or no relation to specific serological reactivity, while the second factor of correction, homologous titer, would perhaps better be treated by dilution of the sera to comparable reactivities.

*The ring test.* In performance of the ring test, tubes of relatively narrow bore (usually 3–4 mm.) are employed. A small amount of the heavier liquid is pipetted into the tube, and the lighter liquid pipetted carefully along the side of the tube so as to avoid mixing the two as far as possible. Ordinarily the amounts of serum and antigen-dilution are not measured accurately. Using the ring test, the antigen, which is customarily the lighter, is better diluted, while the heavier serum is kept constant in concentration. Gilg and Schürhoff in Berlin have called this the "capillary method", but the expression is misleading and should be abandoned because the tubes used (101) are 2–3 mm. in diameter and, thus, far from being capillary. The ring test is read as a rule at 20-minute intervals for 1–1½ hours at room temperature.

The Berlin school of investigators have found much fault with the flocculation

test as used in Königsberg, their chief criticisms being that the readings are too subjective as compared with those of the ring test, and that using Mez' prescription the normal serum controls very frequently show cloudings such that the experiment must be rejected. Later investigators are divided on this question, but an analysis of these points of difference brings out a lack of fundamental disagreement.

In the first place, Mez uses a length of time of incubation of serum-antigen mixtures far in excess of that usually employed by animal serologists. Using the flocculation test, an hour or at the most two hours in an incubator at 37° is sufficient to bring out fully any reaction which might occur. With 12 hours' incubation it is not suprising that artefact, normal serum reactions are common. Even serum and antigen alone are likely to auto-precipitate with such incubation. But while 1–2 hours at 37° is sufficient to permit the precipitate to form, it is not enough to allow it to settle sufficiently for an objective reading, and the only advantage of the last 10 or 11 hours of incubation is to allow the precipitate to settle. This last is accomplished to far greater advantage in a cold room, at a few degrees above freezing. Even the Berlin workers observed no normal serum artefacts before 6–7 hours at 37°, and using the combination of 1–2 hours at 37° + 12 hours on ice, such normal serum artefacts are not usually observed.

As to subjectivity, the chief factor involved is the cloudiness of unsettled, non-specific precipitates in experimental or control tubes according to Mez' technique. Using the combination method described above, such cloudings occur but rarely, and the observed result is in the writer's opinion actually more objective than with the ring test, where the slightest irregularity in pipetting will alter the appearance of the rings.

It is convenient, although not commonly in practice, to perform the precipitin test in tubes about 5 x 50 mm. in size; .2 cc. each of serum and antigen dilution, accurately measured, are convenient amounts to use. It is a good practice to overlayer the two components in such tubes, read the ring test after 1 hour at room temperature, then mix and incubate 1 or 2 hours at 37°C., and then 12 hours in the cold, after which the flocculation reading is made. Although the ring test gives a useful preliminary reading, the final flocculation reading appears to be more decisive. Mixing of the contents of such small tubes may be accomplished with an apparatus made by removing from an electric door-bell the bell and hammer, and attaching a rubber stopper to the hammer-arm. The tubes are held momentarily against the vibrating stopper.

*Mez' "conglutination" reaction.* The "conglutination" reaction, so-called, has come to occupy a very important place in the Königsberg school of serodiagnosis and a word should be said regarding its performance and significance. The term "conglutination" has been used with three very different meanings in serology, and an effort should be made to restrict it. "Conglutination" was first applied to the normal agglutination of blood corpuscles by such substances as ricin, abrin, and crotin (e.g. 227, 28), which at a relatively high concentration cause a clumping of the corpuscles of many species of animals. Later Bordet, Gay, and Streng (Bordet and Gay, *Ann. Inst. Past.* 20, 1906; Bordet and Streng, *Ztbl. Bakt.* I, 49:260, 1909, *reprinted in* Bordet, J., Studies in immunity, Transl. F. P. Gay, *New York* (Wiley) 1909) described a reaction which they designated as "conglutination". Normal horse serum contains a weak agglutinin for corpuscles of various species. The addition of horse serum, corpuscles, and complement, however, produces at best

only a very weak reaction. To the system is added heated (56°) bovine serum, containing a substance called "bovine colloidal substance", or later, "conglutinin", which stimulates the complement to function sufficiently to induce powerful agglutination ("conglutination") often followed by hemolysis. The "conglutinin" is satisfactorily shown to be neither antibody nor complement, but merely accessory to complement, and is peculiar to bovine blood.

Although Mez has taken over the term "conglutination", his reaction bears no relation to either of the above (cf. discussion in 194). The Mez reaction is performed as follows (210, 211):

> To tubes containing 1 cc. of plant extract are added respectively .08, .02, .01, .005, and .000 cc. of immune serum. The tubes are shaken and incubated 2 hours at 37° during which time the mixture is said to become "sensitized". A precipitin reaction does not take place because of the small amount of serum used. Then .4 cc. of fresh beef serum is added to each tube and the tubes are returned to the incubator. A precipitate soon appears, and is read at 20, 40, 60, 90, 120, and 150 min. from the time of the second mixing. The usual types of controls are employed, and the experiment is discarded if any show turbidity. Ziegenspeck (390) later suggested using as serum series: .1, .05, .025, .015, .0625, and .0000 of serum in place of the older series used by Mez. The beef serum must come from a healthy animal because otherwise protective ferments cause a clouding of the controls. Reading is difficult and special illumination of the tubes is recommended.

Mez looks upon this reaction only as a technical variation of the precipitin reaction ("Sie liefert aber die gleichen Ergebnisse und wird sich wahrscheinlich nur als methodologische Variation herausstellen." . . . 211). And indeed this is true as Mez performs the reaction, the beef serum yielding some substance which increases the reaction without apparently changing its specificity, perhaps only adding colloidal material which suffices to make a subvisible precipitate visible. But to look on this reaction of Mez' as homologous with the reaction of Bordet, Gay, and Streng is an error. Manteufel (194) suggests that this be called the "Mez reaction", reserving the term "conglutination" for the Bordet-Gay-Streng reaction, and this seems justifiable especially as the original use of the term for the agglutination of corpuscles by toxalbumins has never come into general use.

With but rare exceptions, Mez' reaction in Königsberg has yielded results entirely comparable with those by the flocculation technique. The rare cases of disagreement are usually traceable to an error in procedure and are discarded. Mez insists that both reactions be performed on all materials for relationship study, and that the two reactions must agree. In Berlin the method has been criticized by Helwig (123), Franz (96), Eisenträger (74), Wermund (371), and Zarnack (387), all on the grounds either that it was impossible to obtain error-free results because of clouding of control tubes, or that if well-controlled results were obtained they did not agree with the results of the ring test. This failure in confirmation may in part at least be due to the fact that the Berlin techniques did not follow those of Mez strictly with particular reference to preëxtraction of antigens, and hence the work may not be comparable. Sauli working independently in Helsingfors, previous to the work of the Königsberg school (309) but using a similar technique found the method to be entirely satisfactory and more delicate than the precipitin technique. The writer has had occasion to use Mez' reaction in parallel with the ring and flocculation tests of plant virus material (50). The technique of Mez was followed closely. The results were entirely comparable in all three tests, and there was no trouble with turbid controls, but the Mez reaction was no more

sensitive than the other two, and that being so it seemed preferable to avoid the additional chance of error when with Mez' technique another variable is added (beef serum) to an already complex system.

### IV. ANAPHYLACTIC REACTIONS

The anaphylactic reactions have been used frequently in plant serology, the various forms of the reaction all being dependent upon the fact that if the inoculation of an animal with a small amount of protein is followed 2–3 weeks later by a second inoculation of the same protein, more or less violent symptoms result, frequently terminating in death. In simplest form the anaphylaxis reaction consists in two such inoculations of the guinea pig, the reaction observed being severe shock following within a few minutes after the second inoculation (gross-allergy test). The sensitizing inoculation may be by various routes, the subcutaneous and intraperitoneal routes being frequently employed, while the second, test inoculation is more frequently by the intravenous or intracardial routes. The amounts of protein required are exceedingly small, and the reaction is highly specific for the protein originally employed.

*The gross-allergy test.* Much of the immunological work on the purified proteins of plants by Wells, Osborne, and their colleagues was performed by making use of the gross-allergy test (361, 365, 366, 364, 374, 367, 368, 369, 382, 140, 175, 263). The amounts of protein required for sensitization of guinea pigs were of the order of .001 gm. to .0001 gm. injected subcutaneously, and after 18 days severe specific reactions usually resulted from the subcutaneous injection of .1 gm. Some of the plant proteins used were so highly active that but .000,000,5 gm. (squash-seed globulin, 364), or even .000,000,1 gm. (edestin, 364) was necessary for cutaneous sensitization. Certain proteins such as gliadin, on the other hand, were much less active, presumably because of their poor solubility in the body fluids (361). Tomcsik and Kurotchkin (348) have shown that anaphylactic shock may even be obtained with a purified carbohydrate from yeast, provided the animal is sensitized passively with serum prepared from whole yeast extract.

Many other investigators have used the gross-allergy test with the more complex extracts of plants, such as the grains and legumes (287, 298, 12, 349, 125, 132), and the fungi (60, 84). The reaction has been used to determine the presence of contaminants in fodder (315, 314), and for the identification of oils through the type of their protein contaminants (194, 351). On the whole, the reaction, while entirely useful, is not considered quite as efficient and delicate as the uterine-strip anaphylactic reaction or the complement fixation test, the chief disadvantages being that an animal can be used but once, and that the observation of the reaction may be obscured by other physiological phenomena, e.g. the direct toxic effect of the foreign substance.

*Temperature, local skin, and bronchospasm reactions.* In order partly to avoid this last difficulty, it has sometimes been the practice to keep an accurate temperature record of the animal at the time of the second inoculation, the temperature fluctuations being indicative of specific reaction. This technique has been used to some advantage in connection with plant antigens by Kōketsu (153), although other techniques were preferred because of the primary toxicity of many plant antigens. Coons and Strong (60), on the other hand, had no success with temperature as an index of shock.

Link and his associates (180) have reported the use of local skin tests for a study of allergic reactions to fungous extracts, and as is well known this type of test is one of the leading techniques used in the detection of specific sensitivities of hay-fever patients. Given an anaphylactic hypersensitivity to a plant protein, the rubbing of that protein into the skin results in a local inflammation; the reaction is highly specific. Thus the nature of hay-fever sensitivity is determined by making skin tests of possible sensitizers, until the specific one is found. The conjunctiva of the eye and the mucus membranes also show a high degree of hypersensitivity and may be used in the detection of specific sensitizers (68, 382, 57, 69). Workers who handle satin-wood (*Fagara flava* of the Rutaceae) show a similar type of local reaction to products of this wood (360). The use of skin tests in detection of the cause of hay fever, the nature of dermatomycoses, etc., will be discussed in a subsequent section.

A third form of anaphylactic reaction is the bronchospasm test, used by Wells and his associates in the study of purified vegetable proteins. The reaction is based on the fact that the second administration of antigen to a sensitized animal gives rise to acute, involuntary spasmodic contractions of the smooth muscle of the respiratory tract. These are recorded on a kymograph drum.

*Passive anaphylaxis.* Passive anaphylactic sensitization is attained a few hours after the intraperitoneal or intravenous inoculation of a guinea pig with serum immune to a plant protein or tissue extract. The method has been used with success by a number of workers in plant serology, viz. Wendelstadt and Fellmer with legumes (370), Karasawa with legumes and grains (143), White and Avery with edestin (374), Lake and others with hordein and gliadin (168), Fellmer with fungus extracts (84), Dunbar with hay-fever serum (69), and Moritz with legumes (234). The usual dosage is approximately .5 to 3.0 cc. of immune serum followed 24 hours later by .05 to .1 gm. of purified protein or 1–2 cc. of plant extract at moderate dilution. White and Avery (374) find the method even more sensitive than active anaphylaxis.

*Schultz-Dale technique.* The most sensitive and satisfactory form of anaphylactic test is that devised by Schultz and Dale. Adequate descriptions of the rather exacting technique will be found in Dale, H. H., *Jour. Pharmacol.* 4 (1912), Dale, H. H., *A System of Bacteriology* (*Med. Res. Counc.*), 9:229–235, and with respect to plant antigens, Moritz, O., *Planta* 7 (1929): 759–814. Briefly the reaction is performed as follows:

> Virgin female guinea pigs are sensitized with plant protein, actively or passively, in the usual manner. For active sensitization the pigs should not weigh more than 125–150 gm., for passive, 250–300 gm. After the customary incubation period (3 weeks active, 24 hours passive) the animal is stunned by a sharp blow at the base of the skull, the throat is cut, and the blood drained for a few minutes. The abdomen is then opened, the viscera laid aside, and the two horns of the uterus dissected out. To each end of each horn is attached a thread. Each horn is then placed in a Ringer solution bath at 37°, the lower end of the muscle fixed rigid, the upper end attached by a lever to a kymograph needle. The muscle is continuously aerated by a stream of oxygen or air. After about half an hour of rest, a quantity of non-toxic liquid antigen is introduced into the bath. A positive anaphylactic reaction is manifested by a rapid contraction of the muscle, which slowly relaxes again. As the same muscle will react successively several times to different antigens, the reaction may be used as an absorption test.

A word is necessary as to the use of plant antigens with such a technique. Many plant antigens are more or less toxic to muscle and will induce non-specific

contractions of the muscle even when the antigens are at low concentrations. The writer found this particularly true of Solanaceous extracts, which in dilutions of 1:1000 or more caused such non-specific reactions as to render the technique useless without their elimination (53). A satisfactory technique consisted in a few hours' dialysis of such extracts against Ringer solution, in collodion ("cellophane") bags with continuous agitation. Such dialysis serves the several purposes of removing crystalloid toxic material, of replacing for the unknown and variable salt constitution of the antigenic extract the balanced salt constitution of Ringer solution in which the muscle is bathed, and of establishing a constant pH in muscle bath and extract. With the added precaution of warming the extracts to 37° before adding to the bath, it is possible to use an antigen concentration (in terms of whole plant sap) of 1:10 without production of artefact reactions.

The Schultz-Dale technique has been used most extensively with respect to plant antigens by Moritz (230, 232, 233, 234, 235, 24, 236, 237, 238), particularly for the purposes of studying the genetic constitutions of plant hybrids, the movement of proteins in plants, and the systematic relationships of plants. It has also been found to be of service in plant serology by Elmore in studies of algal diagnosis (81) and by Wells and his associates (175) in their immunological studies of purified plant proteins. It has been particularly useful in connection with the purification of plant viruses, since the viruses themselves show no anaphylactic reaction, while the ordinary contaminants of purified virus preparations are highly anaphylactic. Recrystallized tobacco mosaic virus protein has thus been shown to contain non-virus proteins of the healthy tobacco plant (53).

### V. LYTIC REACTIONS

*Normal hemolysis.* Plant extracts frequently show the ability to hemolyze the washed red corpuscles of various types of blood. This is true of many of the so-called "phytotoxalbumins" (see discussion and literature references in Part II) and of extracts of numerous plants such as *Datura* (76), *Salpiglossis* (76), *Sapindus* (148), *Agrostemma* (148), *Herniaria* (148). *Yucca* (148), and tomato (50). Even methyl alcohol extracts of numerous species yield normal hemolysins, e.g. *Salix*, *Larix*, *Eucalyptus*, *Ginkgo*, *Paulonia*, *Podocarpus*, *Evonymus*, and *Brassica* (246), the active principle being attributed to chlorophyll and xanthophyll. On the other hand, hemolysins have been searched for but not found in numerous other species of plants.

The hemolytic titers of these normal plant hemolysins are often exceedingly high. Thus cyclamin lyses corpuscles completely at 1:100,000, and partially at 1:285,000 (350). Other plant substances which have titers of 1:100,000 or higher are quillaja acid (350), solanin (271), phallin (148), crotin (79), and digitonein (148). Such titers are customarily determined by adding 1 cc. of plant extract dilution to 1 cc. of washed corpuscles of various species, the corpuscles being at a concentration of 5 per cent (or better 2 per cent) in isotonic saline suspension. Hemolysis is observed after one or more hours at 37°.

*Anti-hemolysins.* If an animal be carefully immunized with the plant hemolysins, antibodies are frequently produced which are capable of inhibiting normal hemolysis. Such anti-hemolysins, for example, follow immunisation with ricin and crotin (134, 135) and with extracts of the fleshy fungi (97, 86, 93). The anti-hemolytic titers reach 1:1000 or more.

Even normal blood serum has some anti-hemolytic power as regards the normal hemolysis of solanin, cyclamin, saponin, and digitalin (18, 20). The immunisation and testing for anti-hemolysins are performed according to the customary techniques, the only qualification necessary being that because of the toxicity of such antigens one must begin with very small dosages, increasing as the animal develops resistance.

*Acquired lysis.* As a result of immunisation with particulate plant antigens it might be thought possible to obtain sera which are lytic to such antigens. However, the various attempts which have been made in this direction have not succeeded. Elmore found that a chlorolysis of *Euglena* and other plant cells occurred in the presence of normal human and rabbit serum, but this was not increased on immunisation (80, 308b). The action was associated with the lipoids of the serum and appeared to require lipoids in the cells. Similar results have been obtained with pathogenic and non-pathogenic fungi (56, 192, 34). In the cases mentioned, however, a production of acquired agglutinins or of protective substances did occur. Cao (42) reported an unusual type of lytic effect of starch grains following immunisation with starch, the lytic effect being determined by titration with Fehling's solution. He found specific combinations more lytic than heterologous combinations, but the work requires confirmation, as the starches in general are not considered to be antigenic. Sherwood (326) found that while neither normal serum nor extracts of plantain leaves suffice to liberate pigment from washed chloroplasts of legumes, the combination of normal serum and plantain extract does produce such a lytic effect. This peculiar result has not been explained.

*Abderhalden reaction.* In the Abderhalden reaction, immune serum and antigen are mixed and placed inside dialyzing membranes, and the appearance of protein cleavage products in the diffusate, as determined by some delicate indicator such as "Ninhydrin" (tri-keto-hydrindene hydrate), is an index of the extent to which the serum has broken the antigen into simpler cleavage products. The reaction is delicate and difficult to perform. Controls are very important, the dialyzing membranes must be carefully tested, and according to most authorities the technique is not generally practicable.

The Abderhalden reaction has been used to detect acquired lysins to plant antigens in very few cases. Elsesser (82) found that in general sera immune to purified vegetable proteins reacted more strongly by this technique with the specific protein used in immunisation than with heterologous proteins, and if closely related antigens were used a relationship reaction was seen. However, exceptions to this situation occurred, and the technique seemed inferior to other serological techniques applied to similar material. Nitzescu (256), attributing pellagra to a maize intoxication, found protective lysins for zein (the alcohol-soluble protein of maize) in the bloods of 56 pellagra cases, while 12 healthy bloods failed to give the reaction, but Herzfeld (124) showed that many normal bloods also give positive Abderhalden tests with zein. Ishiwara (133) performed a number of Abderhalden experiments using denatured plant proteins as antigens. Although he succeeded in differentiating some of these, the results showed very poor and erratic specificity. On the whole, the results with the use of the Abderhalden technique on plant proteins are indecisive and the technique is not recommended for use in plant serology without further preliminary study.

*Complement fixation.* The technique of

complement fixation has frequently been used, and with success, in the study of plant serology. The arrangement is as follows:

1. Hemolytic system: Anti-sheep-corpuscle serum + washed sheep corpuscles → lysis of corpuscles.

2. Fixation system: Inactive anti-plant serum + plant antigen → union of antigen and antibody.

3. Complement: Fresh guinea pig serum.

Reaction 1, which is the observed effect, can occur only in the presence of complement, which is present in fresh serum but not present in the hemolytic system above, because complement is destroyed at 57°C. and the hemolytic serum has been heated to that temperature (inactivated). Complement has been similarly removed from the fixation system.

In performing the test, the materials for reaction 2 are mixed and incubated in the presence of a minimal amount of complement. If reaction 2 is positive, i.e. if one is dealing with an homologous serum and antigen, the complement is fixed or bound, and none is available as free complement. When the hemolytic system is then added, the sheep cells are not hemolyzed. If, on the other hand, the immune serum and plant are not homologous, they do not combine, complement is not utilized, and remains free permitting the lysis of the corpuscles of system 1. The following details of technique have been found practical in complement fixation with plant antigens.

Anti-sheep serum can be prepared by inoculating rabbits intravenously with daily inoculations of 1, 2, 2, 1, 2, and 2 cc. of 50 per cent washed sheep corpuscles in physiological saline suspension, a 5-day rest period elapsing between the 3rd and 4th inoculations. The animals are bled several days after the 6th inoculation. The serum is inactivated at 57° for 1/2 hour, diluted to 1/2 with glycerine, and stored on ice. Sheep corpuscles are prepared by defibrinating fresh sheep blood, washing and centrifuging with an excess of saline 4–5 times, and storing on ice where they keep satisfactorily for several days. The titer of the serum is determined by titrating with sheep cells + an excess of complement. The unit of anti-sheep serum is the minimal amount which will completely hemolyze a 2 per cent solution of sheep corpuscles, and 2 units are used in routine tests. Using 2 units of serum, complement is next titrated, the unit value established, and twice this amount used in subsequent tests. Complement must be freshly prepared and retitrated frequently, but the titer of the serum is unlikely to change with storage.

The plant antigen and the plant immune serum respectively are next added to sheep cells + complement, as well as to sheep cells + hemolytic serum + complement, respectively, to determine the highest concentrations at which these reagents will neither fix complement by themselves nor hemolyze corpuscles. In all future tests the concentrations of plant serum and plant antigen must be below these concentrations. Finally, in the test itself, plant immune serum and plant antigen are added to complement, incubated, and then is added a mixture of hemolytic serum and sheep cells. In all dilutions physiological saline is used as diluent; distilled water is hemolytic in itself. An extensive system of controls is necessary, since any one of the various components used may in itself either be hemolytic or inhibit hemolysis. Customarily the test is performed in Wassermann tubes which accommodate 4–5 cc. of fluid. A convenient arrangement is as follows: Inactive plant immune serum (.3 cc. at 1:10) + complement (.2 cc. fresh guinea pig serum at 1:15) + antigen (1 cc. at various dilutions), incubation for 1–2 hours at 37°, + .5 cc. sensitized sheep cells (1 part hemolytic serum dilution + 1 part 4 per cent washed sheep cells, incubated 1/2 hour at 37°), further incubation for 1 hour at 37°. A more extensive account of the details of the test is given in Taliaferro, W. H., *The Immunology of the Parasitic Infections*, *New York*, 1929.

Complement fixation is the most sensitive of all immunological techniques, but as yet it has not come into general use with respect to plant antigens, partly because of its complexity and unfamiliarity to plant workers, partly because the narrow specificity exhibited is not always suitable to plant relationship study. Yet wherever it has been used with plant materials the results obtained have justified the added labor and difficulties involved. This is particularly true of its uses in the study of fungi pathogenic to man and other animals, of the plant viruses, and of purified plant proteins, and the success thus far obtained justifies a more extensive usage in this field. To

be sure, the plant antigens present difficulties which are of greater importance with plant materials than with animal and bacteriological antigens. As has been noted in the preceding section, many plant antigens are so highly hemolytic in themselves as to preclude their use without further purification. Others in themselves are anti-hemolytic. Since a number of these disturbing principles are non-proteinaceous, a technique of dialysis through collodion might sometimes serve as a useful preliminary to complement fixation with plant antigens exhibiting such artefact reactions, and in the case of hemolytic plant proteins titration to favorable hydrogen-ion concentrations, such as proved successful in the case of the tomato hemolysin (page 89), might offer a solution to this difficulty.

### VI. AGGLUTINATION REACTIONS

*Normal hemagglutination.* Red blood corpuscles are not only hemolyzed by many plant extracts, but are also frequently agglutinated by such extracts. This applies in particular to the phytotoxalbumins (see discussion in Part II), to the extracts of *Amanita* (88, 91, 93) and other fleshy fungi (94, 95, 97), to purified plant proteins such as edestin (374), and to the non-toxic extracts of many species of plants, (cf. discussion in Part II). Not only are corpuscles agglutinated by abrin, crotin, and ricin, but also pus cells and isolated cells of various animal tissues (173, 223). The titers of these normal plant hemagglutinins are usually much lower than those of the normal plant hemolysins, but have been occasionally ascertained to reach as high as 1:10,000 (edestin, 374) or 1:32,000 (bean extracts, 170). Use has been made of the normal agglutination by ricin in the detection of ricin contaminations in animal fodders, since the ordinary constituents of fodders do not show such an agglutinative action (227). The customary procedure (288) consists in the preparation of a .85 per cent saline extract of the dried plant seed, to which is added a dilute suspension (2 per cent) of washed blood corpuscles. Agglutination is observed either microscopically or with the unaided eye after an hour or more at room temperature.

*Normal serum agglutination of plant antigens.* Conversely, normal serum may cause an agglutination of particulate plant antigens, although the titers are usually rather low. Thus fungus spores and yeast cells are agglutinated by normal serum (41, 197, 240) and by peritoneal fluid (56), chloroplasts are occasionally weakly agglutinated by normal serum (326), and suspensions of gum arabic and plant oils show a reaction resembling agglutination in the presence of normal serum (352). Weak agglutination of *Euglena* cells (titer 1:50) by normal serum has been observed by Elmore (80), and the agglutinative principle was removed by heating the serum to 56°, but Steinecke (333) in a study of acquired agglutinins toward algae was not troubled by an agglutination due to normal serum.

*Anti-hemagglutinins.* By immunizing animals with the normal plant hemagglutinins, sera are often produced which inhibit the normal hemagglutination. This has been shown for the hemagglutinins of ricin (134, 135), abrin (287), edestin (374), and bean extract (287), although attempts to produce anti agglutinative sera against the *Datura* (76) and *Amanita* (93) hemagglutinins did not succeed. But in the case of the latter at least, the agglutinin is non-protein and of a rather low titer, hence the phenomena may not be comparable. The titers of such anti-agglutinative sera may reach 1:10,000 (edestin, 374). In acquired immunisation the washed corpuscles show

no resistance to agglutination by the specific antigen used, but such resistance as appears is confined to antibodies in the serum (227, 183).

*Acquired agglutination.* The inoculation of animals with corpuscular plant antigens, e.g. unicellular algae and fungi, usually gives rise to the production of agglutinins. In contrast to the other serological reactions with plant antigens, however, the agglutination reaction is usually weak and not highly specific.

Acquired agglutination of algae has been reported by Dunbar (in 300), Rosenblat-Lichtenstein (300), Lieske (176), and Steinecke (333). The highest titers of the immune sera have been reported at 800 (300), 4000 (176), and 12,800 (333). Complement is held to be unnecessary to the reaction (300). Green and colorless cultures of the same alga show limited (176) or no (300) cross reactivity, the reaction hence not appearing to be bound up with the species-specific elements of the protoplast. Either living or dead (56°) algae may be used and give identical results (176). The method is held to be unsatisfactory with large thick-walled algae.

As regards the fungi, acquired agglutinins have been demonstrated in the cases *Aspergillus* and *Mucor* spores (197, 41, 307), pathogenic and non-pathogenic yeasts (321, 323, 240, 64, 30, 193, 270, 344, 178), and species of *Monilia* (145, 178, 83, 126, 177, 207, 224). In general, such agglutinins have been produced by the artificial injection of laboratory animals, but they have also been detected in the blood of patients suffering from diseases due to pathogenic fungi (224, 83). The titers are often very low, of the order of 1:20 or 1:40 (41), 1:100 (240), 1:50 (307), 1:80 (64), and 1:90 (192), or less frequently up to 1:500 (145) or 1:5120 (207). Normal serum, as has been seen above, will not infrequently agglutinate such plant antigens at titers but little below these. Complement has been found to be unnecessary to the reaction (323). Either living or dead fungi may be used for immunisation (240), and yeasts may be heated even to 115° without interfering with the reaction (64, 193). The yeasts are not agglutinated in general by the non-specific substances which agglutinate bacteria, such as formalin, $HgCl_2$, and dilute acid and alkali, but acetic acid does agglutinate them (193). Minute amounts of calcium have been found to be essential to the reaction with yeasts (30).

It is very probable that in the agglutination of the algae and fungi the capsular substances are most important, the protoplast itself having little part in the process. This is in keeping with the facts that the yeasts may be strongly heated without altering their antigenicity, and that empty plasmolyzed and washed yeast capsules produce sera giving agglutination reactions identical with those prepared from whole yeast cells. An interesting mechanism of agglutination is seen in *Euglena gracilis*. When Sauer (308a) immunized rabbits with this alga she found in the serum a specific cytotoxin which caused the *Euglena* cells to discard their flagella and settle to the bottom.

The specificity of the agglutination test with these plant antigens is by no means as sharp as that with tests such as the precipitin and complement fixation reactions. Thus little or no differentiation has been obtained between species of *Monilia* (207) or between *Monilia psilosis* and the yeasts of sprue and blastomycosis and other yeasts (126). An interesting case of lack of species-specificity is that reported by Sugg and Neill (344), who obtained cross-agglutination and -protection tests between anti-yeast serum and pneumococcus Type II. This lack of species-speci-

ficity of the agglutination reaction with plant antigens may well be due to the same factors as have led to the view expressed above that the reaction in this case is one involving the relatively non-specific capsular substances and not the proteins of the protoplasts.

Uhlenhuth and Jung (352) have described a phenomenon resembling acquired agglutination occurring when immune serum was added to a suspension of olive oil and gum arabic or rubber sap. The reaction did not occur in the presence of normal serum, and it was not specific for the olive oil, other oils reacting with equal facility in the presence of anti-olive-oil serum. It is very likely that in this case the pure oils in themselves were not antigenic, in the sense of immunizing, but that in the presence of protein admixtures or contaminants immune sera were produced which reacted with the oils employed (haptene or "Schlepper" theory). Citron (56), while unable to demonstrate acquired agglutinins to a species of *Trichophyton*, observed what he considered to be an agglutination of the fungus *in vivo* in the peritoneal fluid.

The technique of agglutination testing consists in the preparation of immune sera preferably by intraperitoneal injection to avoid the danger of embolism following intravenous injection, and the addition of a small amount of serum dilution to a fairly dilute saline suspension of antigenic material, the observation being made of the sedimentation in the test tubes, or perhaps more favorably of the clumping as observed through the microscope on a hollow-ground slide.

### VII. ACQUIRED RESISTANCE TO PLANT PATHOGENS AND TOXINS

Besides the demonstration *in vitro* of immune reactions in animals toward plant antigens, there is a considerable body of evidence showing that there may be acquired a resistance to toxic plant products or toxic antigens from pathogenic plants, demonstrable *in vivo*, and that such acquired resistance may be accompanied by or due to the production in the animal of immune substances which inactivate or neutralize the toxins, or kill the pathogens involved.

*Acquired resistance to plant toxins.* The injection into animals of progressively increasing sub-lethal doses of plant toxins frequently produces an immune state such that doses of the toxin far in excess of the maximum sublethal dose for a normal animal may be borne with impunity. This has been demonstrated for the residual toxins in cultures of pathogenic yeasts (305) and *Actinomyces* (252), for the phytotoxalbumins, ricin, abrin, crotin, and robin (cf. Part II), and for the toxins of *Amanita* (86, 88). The toxins mentioned above, with the possible exception of one component of that from *Amanita*, are proteinaceous. On the other hand, animals which have recovered from sublethal doses of the highly poisonous crystalline glucoside of the "Tutu" plant (*Coriaria thymifolia* and *C. ruscifolia* of New Zealand) show no subsequent resistance (92).

While an immunity to the toxins of *Actinomyces* is held to be accompanied by an immunity to the fungus itself (252), this is denied for pathogenic yeasts (305). Immunity to plant toxins may be acquired either by injection or by feeding of the toxin, and the degree of immunity is frequently fairly high. Thus it has been shown that rabbits immunized with ricin either *per os* or by subcutaneous injection may later resist as much as 400 to 600 minimal lethal doses (134, 135) and the same has been shown for abrin (72). With abrin (57) it has been observed that the immunisation of the conjunctiva of the

eye is followed by a local immunity confined to the immunized eye, which later develops into a more general immunity toward this toxin. Acquired resistance to ricin may last as much as 7½ months after immunisation (72).

*Protective, neutralizing, and lethal substances in the blood of animals immunized with plant toxins or pathogens.* Neutralizing antibodies specific for the toxins used in immunisation have been detected in the blood of animals immunized with a number of the phytotoxalbumins (cf. Part II). Furthermore, the immunisation of animals with certain plant pollens results in the production of sera which are antitoxic to the hay-fever toxins contained in such pollens (68, 69). The titers of such sera are reasonably high, e.g. neutralization of 100 normal minimal lethal doses of ricin by immune rabbit serum (183). The antitoxin may persist in the blood for some time after immunisation (72). The neutralizing antibodies for ricin and the *Amanita*-toxin are present in the blood stream but are not demonstrable in the serum-free red corpuscles (183, 134, 86).

It is questionable whether the sera of animals prepared by immunisation with yeasts have a lethal or inhibitory effect on the yeast cells (193, 30). This is not unexpected, since, as was seen above, the antibodies produced by the injection of yeast cells are directed at the wall substance of the cells and not at the protoplasts themselves.

Sera prepared against pathogenic species of *Trichophyton* (56), *Saccharomyces* (305), *Aspergillus* (61), and *Monilia* (177) in no case protected other animals against single minimal infective doses of these pathogens, and in the case of *Actinomyces gypsoides* only an incomplete protection has been claimed (252).

*Acquired neutralization of the plant viruses.* In the earlier literature on the serology of the plant viruses there have been several preliminary reports of a neutralization of tobacco mosaic virus by immune rabbit serum (244, 279, 280, 200, 328, 106). Normal rabbit serum, however, also was seen to have a strong inhibitory effect on virus infectivity, and accordingly it was not clear whether the effect of immune serum was actually qualitatively different from that of normal serum. It has recently been shown (47) that normal serum inhibits virus infectivity not principally through an effect on the virus but through a decreasing of the susceptibility of the host plant, while virus-immune serum in addition has a neutralizing effect on the virus. This neutralization is entirely specific as between the viruses of tobacco mosaic, tobacco ring spot, and cucumber mosaic, and the reaction follows the law of multiple proportions. Time is required *in vitro* for its demonstration. The serum titers are relatively high, 1:2,500 or more. Stanley has recently shown (332b) that virus protein which has been inactivated so that it no longer possesses infectivity will still elicit neutralizing antibodies in rabbits. If neutralized mixtures of tobacco mosaic virus and its specific antibody are digested with pepsin, the antibody is destroyed and the virus antigen recovered, while if similar mixtures of latent mosaic virus and its antibody are acidified, the converse is true, the virus being destroyed and the antibody recovered. Thus in these cases of neutralization the virus and antibody, respectively, are not destroyed but are held in a latent condition from which they may be freed by suitable treatments (55).

### VIII. OTHER SEROLOGICAL TESTS WITH PLANT ANTIGENS

A number of other less usual serological techniques with plant antigens have been attempted, in some cases with positive

results, and these are here summarized briefly.

*Phagocytosis and opsonic index.* Skchiwan (329) has noted that the injection of yeasts into the peritoneum of rabbits is followed by an attempted ingestion of the yeast cells by leucocytes. The yeast cell responds by secreting a thick protective membrane, and retains its viability for several days. In intravenous injection, however, the yeast cells are quickly killed by phagocytes. Similarly, Citron (56) found that leucocytes protect normally against injected *Trichophyton*. The inoculation of pathogenic yeasts, according to Peckham (270) is followed by an increase in phagocytic activity as measured by the opsonic index, and this acquired reaction is demonstrable before the complement fixation and agglutination reactions appear. Porges (275) claimed that active homologous serum increases the opsonic effect in the phagocytosis of starch, but that this was not increased by immunisation with the starch. Inactive sera, on the other hand, inhibited such phagocytosis. These last results are difficult to interpret with no more than the scanty data offered regarding this apparently complicated condition. The increase in opsonic index with immunisation by bacteria is a regular concomitant of the acquired serological reactions, and although this reaction would have no general applicability with the plant antigens, it would be instructive to have more evidence to add to the limited findings noted above, with particular regard to the yeasts and yeast-like fungi, the unicellular algae, and possibly the pollens of plants.

*Effect of immune serum on the germination of seeds and the growth of plants.* Raubitschek (287) has reported that sera immune to the lentil and bean proteins often check the germination or growth of the homologous seeds or seedlings in a specific fashion. However, many of the sera obtained by him failed to exert such an action, and furthermore Kubeš (165) found that immune serum was no more effective than normal serum in preventing the germination and growth of poppy, clover, bean, and other plants. The scanty data thus far adduced accordingly do not furnish any reliable evidence that immune sera are able to act in the manner in question.

*Decoloration of plant dyes by dye-immune sera.* De Angelis (5) has claimed that the injection of animals with the plant dye hematoxylin (as well as with methylene blue) results in the production of sera which not only precipitate solutions of this dye but also decolor the solutions. The work has been repeated by Takemura, however (345), and no evidence was found that such a decoloration occurs on the addition of hematoxylin-immune serum to solutions of the dye, nor indeed were any precipitins demonstrated.

*Pfeiffer's test.* Lieske (176) in a serological study of the algae attempted the application of the Pfeiffer test to his material. Briefly, the test consists of a lytic demonstration in which organisms and immune sera are injected into the peritoneum of guinea pigs and the peritoneal exudate is later studied microscopically. He was unable, however, to obtain useable results by this method, although his sera were satisfactory as shown by other types of serological tests.

*Anti-diastatic power of yeast-immune sera.* Malvos (193) observed that sera from animals immunized against yeasts had no power of restricting or inhibiting the diastatic activity of the yeasts.

*Coagulating effect of plant toxalbumins on normal blood and other animal proteins.* In addition to the hemagglutinating, hemoprecipitating, and hemolytic property of the phytotoxalbumins it has been found that ricin, abrin, and crotin also stimulate

the coagulation of normal blood (122, 173). Cytisin does not exert such an effect (285). Likewise abrin coagulates normal cerebrospinal fluid (122), and crotin, ricin, and abrin curdle milk, although they have no such effect on such other animal proteins as myosin, meat-broth, and ovarial mucin (173).

*(To be continued)*

# SURVIVAL OF THE ORDINARY

By W. L. McATEE

## INTRODUCTION

NATURAL selection theory has been so frequently criticized that it seems well nigh impossible to bring forward an entirely new argument. Perhaps by presenting matters in a fresh light, however, and even by iteration alone, the attention of open minds may be focussed on some of the fatal weaknesses of the theory.

The thesis of the present paper is not original but it was independently reached and has been the subject of reflection for years. I find it fairly well stated in a letter written in 1917 from which the following quotation is taken.

> "I am unable to put much faith in natural selection however, and none in that phase of it expressed by the phrase 'survival of the fittest.' It seems Quixotic to assert, that among the very large numbers of offspring, produced by most animals, only the fittest survive. Chance enters into the equation so largely that the fittest stands a proportional chance of being the first eliminated. It seems to me that the survivors will almost invariably come from the great median group of ordinary specimens, and not from either the small proportion of subnormal or of supernormal individuals. In other words, natural selection will usually leave typical specimens to reproduce a species, and is a conservative rather than a progressive process."

About the only change I would make in that statement today would be to quote "natural selection" as well as "survival of the fittest," as both of these terms are are mere slogans used almost invariably without the slightest analytical perception. A fair interpretation of typical assertions might read: "Mortality occurs; there are survivors; natural selection therefore has been effective, and the survivors are the fittest." As has been pointed out on various occasions this is quite irrational, and means nothing but that survivors survive.

## "SURVIVAL OF THE FITTEST"

Darwin uses the phrase "survival of the fittest," or equivalent words, less than half the times he defines the expression "natural selection," yet on those occasions he does it so deliberately and definitely that we must assume the synonymy was satisfactory to him. For instance near the beginning of Chapter III of the "Origin of Species" he says: "I have called this principle by which each slight variation, if useful is preserved, by the term natural selection in order to mark its relation to man's power of selection. But the expression often used by Mr. Herbert Spencer, of the Survival of the Fittest, is more accurate, and is sometimes equally convenient." (1912?, 55).

In this statement we have the implication that a slight useful variation puts its possessors in the ranks of the fittest, while as a matter of fact it would certainly place them in the median or average class to which considerable attention is given later. It would take more than a slight favorable variation among omnipresent slight neutral variations to move an organism from near the median state of its kind to the superior status implied by the word "fittest."

For the present, however, let us contemplate the meaning of the full phrase "survival of the fittest." *Fit-test* is a superlative; it means the best, the very best. As a prime consideration we must not forget that organisms are prolific,

many of them prodigally so, yet no matter how large the number of offspring, only enough survive on the average in each generation to replace their parents. Suppose this number is two as it is in so many cases, then "survival of the fittest" means that the very best two out of a usually large, often enormous, number of offspring are "selected" to perpetuate the race. What the probabilities of this outcome are may be judged to some extent from the mere numbers involved.

### FECUNDITY OF ORGANISMS

Statements of the numbers of seeds, eggs, or young are available in many places but it will do no harm to cite examples. Prof. O. A. Stevens studied the seed production of numerous weeds and reports (1932) the number of seeds produced by an average individual plant to be 1000 or less in the case of 23 species, "1000 to 10,000, 86 species; 10,000 to 50,000, 56 species; 50,000 to 100,000, 9 species; above 100,000, 9 species." The highest number revealed by this study was 1,075, 000 for *Artemisia biennis*. A single *Paulownia* tree has been estimated to produce 21,000,000 seeds, while the numbers of reproductive bodies borne by orchids and ferns reach much more enormous numbers as 74,000,000 seeds for a plant of the orchid genus *Acropera*, and spores in the following numbers for ferns of the genera named: *Dryopteris* 50,000,000 to 100,000,000 (from the whole plant in a year); *Aratia* 2,800,000,000 and *Angiopteris* 4,000,000,000 (in each case the produce of a single leaf!).

Turning to the animal kingdom, the egg complement in our common frogs of the genus *Rana* ranges from 2,000 to 20,000; some of the stone-flies deposit from 5,000 to 6,000 eggs; dragonflies 5,000 to 11,000; and ticks 10,000 to 17,000. Presumably all of these are seasonal records; as an upper record for an insect the queen of an African species of termite is reported (Savage, 1850) to lay 80,000 or more eggs daily.

The lobster at its most productive period is said to bear 160,000 eggs at a time. Fishes are noted for their fecundity, and a good many kinds attain or surpass the million mark in eggs; angler 1,000,000 to 3,000,000; cod 2,000,000 to 9,000,000; coalfish 5,000,000 to 8,000,000; turbot 5,000,000 to 30,000,000; and the ling 12,000,000 to 60,000,000. The egg urchin may produce 20,000,000 eggs in a season and some kinds of starfishes twice as many. According to Weismann "Leuwenhoek calculated the fertility of a threadworm at sixty million eggs, and a tapeworm produces hardly less than 100 millions." (1904, 47).

### HIGH RATE OF MORTALITY

However great the number of offspring, there survives on the average only one to replace each parent. The mortality rate, therefore, high in almost any case, is stupendous where such large numbers as those here cited are concerned. The average gross mortality in each generation is the means through which "natural selection" is conceived to operate. Apparently selectionists have assumed that high mortality automatically involves high selectivity. That this is merely an assumption is readily proved by analysis of the process of elimination.

A principle, almost universally evident, is that mortality varies directly with the immaturity of the forms involved. Thus there are, as a rule, heavier losses among ovules and pollen, among unfertilized eggs and sperm, than among any of the products of union of the gametes and there is higher mortality among seedlings and larvae or similar early stages than among adults. In fact until the approach of

senility, maturity brings a proportionate degree of security. The life-histories of almost any organisms illustrate this principle but those of highly prolific forms do so most clearly and emphatically. To those who see in this process only survival of the fittest we suggest the thought that if immaturity is to be interpreted as unfitness then the survival of the fittest evidently is not the ruling principle of evolution as adults regularly give way to young, and in innumerable cases (e.g. insects in general) completely disappear leaving the fate of the species entirely with the immature. Moreover it is not a comforting thought for selectionists that while a very great share of "selection" affects the immature forms, in proportion to the degree that this occurs, there will be just that much less selection of adults upon which continuation of species depends.

### THE PROCESS OF ELIMINATION IN PLANTS

*Oaks.* Beginning again with plants consider the case of the oaks. The first "selection" (an abominably misused term) in the reproductive process each year (or two years) is among the ovules and pollen grains. Do the fittest of these necessarily participate in the formation of the acorn crop? The ovary has three cells, each with 2 ovules, and in maturity 2 of the 3 compartments and 5 of the 6 ovules become obliterated (Lindley and Moore, 1876, II, 948). Here is an elimination of 5 out of 6; in addition the pistillate flowers must share in the casualties to twigs, mentioned later, so that the proportion of survivors among the ovules is small, and their survival a process in which chance plays a far greater part than degree of fitness.

From examinations kindly made for me by A. C. Martin in April 1933, anther sacs of a specimen oak in the Mall, Washington, D. C., averaged somewhat more than 1800 pollen grains each, and each stamen therefore, produced in excess of 3600 grains of pollen. The flowers had an average of 7 stamens and consequently over 25,200 pollen grains, while the aments averaged 33 flowers producing all together in excess of 800,000 grains. This many from a single catkin, and the latter are produced in such profusion that their dried and shrivelled remains after they have shed their pollen and fallen to the ground can be raked up literally by bushels. On a single small twig used in the study referred to, there were 62 aments, the yield of which in pollen grains must have been more than 50,000,000. With that number from one twig, what an enormous number—millions of millions—of pollen grains must be produced annually by a mature oak.

These pollen grains have no direct means of reaching the stigmas but depend on wind, gravity, or perhaps chance disturbances, to scatter them. That the best of them, the actual "fittest," if indeed there are any "fittest," from such a vast number, and dependent on accident for the opportunity to fertilize an ovule, do in fact achieve that fertilization is entirely beyond the bounds of belief.

Even if selection of the "fittest" in the fertilization process is manifestly impossible, let us look into the fate of the fertilized ovules. Some will not mature due to a variety of causes including the attacks of insects (either direct or on the twigs bearing acorns), that can have little relation to unfitness. Squirrels eat off many twigs apparently wantonly, and cause the loss of whatever immature acorns there may be on them without regard to fitness; these and other acorn-eating mammals and birds cannot wait for the nuts to ripen and pick off, and consume in part, or in whole, many a

green fruit. Infestation by nut weevils (*Balaninus*) is common and sometimes reaches a high percentage. These weevils do not select the unfit, they are seeking a safe place and assured nourishment for the development of their own young and on the average, no doubt, oviposit in just as good acorns as there are available.

After all casualties, the formative and growing acorns must survive, in doing which it is apparent that fitness plays little or no part, thousands of acorns are, nevertheless, ripened by every large tree. These drop off during a more or less protracted season and suffer a great variety of fates. Some may fall in holes, or rock crevices where they will never germinate though they be of the best; some may roll into, or be washed into, rivulets and be borne to larger waters where they may perish or perchance lodge in a place where they will germinate. If they have been carried to a place too different from the habitat of their kind they are again subject to elimination, but through no fault of their own, through no lack of fitness. Most of the acorns in normal course continue to lie directly beneath the parent tree, where animals may trample them or devour them. Some may lodge in piles where the uppermost will dry out with no chance of germination. They must perforce remain on the surface over the winter, giving opportunity for many destructive factors to operate, few if any of which are guided by the fitness or unfitness of the acorns.

The destruction of acorns by animals is so great as to be an object of concern to foresters, one of whom, Clarence F. Korstian, has published studies (1927) upon the subject, made in North Carolina, that may be abstracted as follows: The proportions of acorns of four different species destroyed by insects were 9.8 to 27.69 per cent and by birds and mammals 56.51 to 74.78 per cent; these percentages combined showed destruction in the various cases of from 80.6 to 94.62 per cent of the total crop. Additional toll was taken of germinated acorns varying from 1.75 to 4.52 per cent. The proportion of sound acorns and live seedlings together surviving animal feeding varied from 0.37 to 6.65 per cent. The animals concerned were deer, bears, cattle, hogs, rabbits, squirrels, chipmunks, mice, turkeys, crows, jays, blackbirds, nut weevils, moth larvae, and cynipids.

We can be sure that all of these creatures in search of nourishment will eat of the best; in fact it is traditional that squirrels and jays get the cream of the crop. What of the comparatively few that do finally succeed in germinating and taking root? Rabbits, squirrels, or mice may bite them off, or they may be browsed fatally or trampled by deer, accidents they can in no way avert by "fitness." Any surviving these dangers as a rule die through shading out by the parent and other trees. We have not mentioned every hazard affecting the fertilization, the growth and ripening, and the germination of acorns, nor the persistence of seedlings; no man knows enough to do that. We have shown, however, that with respect to the high degree of mortality among ovules, fitness cannot enter into the equation to more than a slight extent. Certainly the "fittest" have no assurance of survival. Fertilization is ruled by chance and from the very numbers involved—millions of pollen grains for every pistillate flower—it is certain that there can be no "survival of the fittest." The elimination of growing, and again of mature acorns, is mostly due to the feeding of animals and they seek the best. These two stages of elimination, both with the very opposite effect to what the theory of "natural selection" demands, amount

to perhaps 50 per cent of the green, and to from 80 per cent to 95 per cent of the ripe acorns. Later comes elimination of seedlings also partly due to agents that allow no play for fitness. Willis refers to disease as a factor of this kind and says that in experimental work with plants "it is disease which kills the bulk of the losers, and that these are not necessarily the weakest but often well-grown plants" (1915, p. 326).

The process of reproduction in the oak, prodigally wasteful of just as good material in the way of pollen, ovules, and fruits as the tree can produce, goes on year after year for possibly two, three or more centuries and in the end (speaking in average terms) there will be left to replace the old tree only a single one of the almost infinite number of progeny it has produced. Can anyone believe that the survival of that one oakling out of untold millions is due to inherent superiority —that it is the "fittest" of all?

*Orchids.* Another group of plants in which the wastage of immature stages is enormous is the orchids. Despite elaborate flower structure dependent on the visits of insects to insure fertilization, many flowers are not fertilized, and the ripening of seed capsules is the exception rather than the rule. The prevalence of vegetative methods of reproduction in the group shows that the fertilization mechanism, the most specialized, on the whole, in the vegetable kingdom (and claimed to have been developed by "natural selection"), is not depended upon as a safe means of reproduction.

It seems therefore that pollen and ovules no matter how numerously produced are as a rule wasted. When seeds are ripened, however, they are characteristically very numerous. One plant of *Maxillaria* has been estimated to produce 10,500,000 and one of *Acropera* 74,000,000 seeds. With such fecundity, if the seeds were in any considerable degree successful in making new plants, the earth would soon be covered with orchids, yet in fact they are as a rule local and rare. The explanation is of course that the seeds do not germinate and grow. They are minute and in many species lose their vitality in a few months. They depend on so erratic an agent of dissemination as the wind, yet to be successful they must reach a spot where not only proper conditions of warmth and moisture prevail, but where there must be also a growth of a more or less specific strain of symbiotic fungus.

The chances against completion of this chain of events are so great that Oakes Ames, a great orchid specialist, was constrained to remark, "For every dozen seeds that fall where endotrophic fungi of the proper type are present, millions must drift to sterile ground and suffer extinction" (1922, 46). Under such circumstances it is evident that fitness can play little part. It is clear that we have instead mass elimination by chance of both fit and unfit in whatever proportions these may exist among the seeds.

*Western yellow pine.*—Conditions favorable to reproduction of the western yellow pine (*Pinus ponderosa*) are frankly stated to be due to chance by G. A. Pearson of the United States Forest Service from whom we quote the following paragraphs:

> "Studies aiming at a quantitative determination of the amount of seed required to secure a satisfactory stand of seedlings have not succeeded in fixing absolute standards, because of the extreme variations encountered on different sites and in different seasons. The greatest variable is rainfall. In unfavorable years no amount of seed will bring results. In seasons when growing conditions are normal or above normal, germination is good if seed is present, and usually there is some survival.
>
> "One of the most direct and obvious factors in seed supply is the consumption of seed by rodents. The effective seed supply is that which is preserved

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until moisture and temperature conditions are such that germination can take place. In years of light seed production chipmunks and mice probably destroy almost the entire crop, but in good seed years a considerable amount remains unconsumed. The surplus is what counts in natural reproduction. (1923, 10)

"In attempting to fix a minimum limit of seed requirement, it must be borne in mind that in average years from 90 to 99 per cent of the seeds will be lost either through failure to germinate or through infant mortality. It is estimated that on the plots under observation near the Fort Valley Experiment Station during the nine years prior to the heavy seed crop of 1918, not more than one out of every thousand germinable pine seeds produced a seedling which lived beyond the age of 5 years. Present indications are that the 1918 seed crop, because of the extraordinary moisture conditions in 1919, will yield much higher returns, possibly as high as 5 per cent and probably not less than 1 per cent. This should result in complete restocking on areas which were well seeded. Heavy crops may fail to give results, because they are not followed by suitable climatic conditions, and likewise a period of favorable climatic conditions may be wholly or partly ineffective, because it was not preceded by a good seed crop. Although the right combination, such as was experienced in 1918 and 1919, appears to be purely a matter of chance, we should aim to take advantage of such opportunities whenever they come." (*ibid.* 10–11)

Mr. Pearson's remarks not only set forth the usual high mortality among seeds of the pine, but demonstrate that survival as he says is "purely a matter of chance." Fitness evidently is of no avail when the entire seed crop is wiped out year after year by unfavorable climatic factors.

### THE PROCESS OF ELIMINATION IN ANIMALS

Let us now examine instances of great fecundity and devastating elimination in the animal kingdom.

*The cod.* This species "is one of the most prolific of fishes, so much so that a female 39 or 40 inches long may be expected to produce about 3,000,000 eggs and one of 41 inches at least 4,000,000. Earll estimated the number of a 52½ inch fish weighing 51 pounds at 8,989,094, with 9,100,000 in a 75-pounder" (Bigelow and Welsh, 1924, 428).

The eggs are minute, transparent, and buoyant; they are liberated at random in the sea as is also the sperm so that there must be a great waste of sex cells. For the eggs that are fertilized, a primary hazard is the specific gravity of the water, and this factor which determines whether the eggs float and develop, or sink and die, must be correct within a few thousandths of one per cent. While adult cod prefer a temperature of from 35° to 42°, and spawn in late winter or early spring, mortality among the eggs is great in the case of what would seem to be almost a normal happening at that season, namely, for the temperature of the water to be as low as 32°. Untold numbers of the eggs may be killed also by sudden changes in temperature, or may be driven ashore by adverse winds and currents.

Even in favorable seasons the eggs float about at the mercy of wind and wave for about two weeks. They are of course perfectly inert and are unresistingly devoured by spawn eaters of numerous kinds. When first hatched the young cod also are pelagic and drift helplessly, a condition in which they may persist for about two months; during this period it is evident that there is no fitness that will fend off enemies.

To summarize, the immature stages of cod under the best conditions are exposed for a period of 10 weeks to elimination by agencies which undoubtedly act upon them with utter indiscrimination. So complete in fact is the elimination that it is a matter of record that in some years there is a practically complete failure to produce young over vast areas though every one of the millions of female cod there spawns its millions of eggs.

*The Atlantic oyster.* The oysters of the east American coast, according to J. H. Orton "shed their sexual products directly into the sea where haphazard fertilization occurs. A good-sized individual may shed at one spawning upwards of 50 to 60 million eggs." (1929, 1004). The immature forms grow rapidly but it is about two weeks before they settle and cement themselves to some clean-surfaced hard object where they develop adult organs and pass the remainder of their lives. Ordinary sea bottom, and objects in the water covered with the almost omnipresent slime, algae, or other marine organisms are unsuitable as places for attachment, or cultch, as they are technically called, and in oyster culture it is very necessary to supply clean, hard, material to enable the oysters to make any gains. "The settling of larvae on objects in the sea is called a spatfall." The eggs may be spawned at a temperature of 3 or 4 degrees lower than is necessary for a good spatfall. Hence if the temperature of the water does not rise that amount during the larval period in seasons when early spawning has occurred, there will be more or less of a failure in spatting. Even in favorable years the larvae are at the mercy of tides and currents and it is by chance alone that some of them find the necessary firm anchorages. The proportion that do thus succeed is normally very small, and it is certain that their success is due more to luck than fitness.

"The oyster's natural enemies [also] are more numerous in early than late stages of life. The larva is eaten by small fishes, jelly-fishes, other bivalves, sea-squirts (ascidians), worms, anemones, and small crustaceans, all of which are usually common on oyster beds. Spat, young, and old oysters are attacked by borers which bore a hole through the shell and eat the oyster through the hole. . . . Fifty per cent of a spatfall may be destroyed where borers are abundant." (*ibid.*, 1005).

There is no fitness involved in relation to the attacks of predators as the oyster bed is for them simply a pasture upon any part of which they feed at random. Not only is indiscriminate destruction exemplified in the case of the oyster, but also very clearly the principle of intensity of elimination in proportion to the immaturity of the stock, as well as that of survival by age classes, intermediate classes being largely wiped out by unfavorable local conditions.

*The human tapeworm.*—The sheer incredibility of the proposition that the fittest among millions survive is well illustrated by the life history of many parasitic organisms. All that needs to be said about a typical example, the human tapeworm, can be quoted from the writings of an ultra-selectionist, Weismann, who informs us that this parasite produces hardly less than 100,000,000 eggs.

"There exists, . . ." he says "a constant relation between fertility and the ratio of elimination; the higher the latter is, the greater must the former be, if the species is to survive at all. The example of the tapeworm makes this very obvious, for here we can readily understand why the fertility must be so enormous, as we are aware of the long chain of chances on which the successful development of this animal depends. The common tape-worm of Man, *Taenia solium*, does not lay its eggs, they remain enclosed within one of the liberated joints or 'proglottides.' Only if this liberated joint or one of the embryos within it happens to be fortuitously eaten by a pig or other mammal can there be successful development, and even then under difficulties and possible failures, and not right away into adult animals, but first into microscopically minute larvae which may bore their way into the walls of the intestine, or, if they are fortunate enough, may get into the blood-stream and be carried by it to a remote part of the body. There they develop into 'measles,' the so-called bladder-worms, within which the head of the tape-worm arises. But in order that this may become a complete and reproductive adult worm the pig must die, and the next step necessary is that a

piece of the flesh of the infected first host must happen to be swallowed raw by a man or other mammal! Only then does the fortunate bladder-worm—swallowed with the flesh—attain the goal of its life, that is, a suitable place to mature in, the food-canal of a human being. It is obvious that countless eggs must be lost for one that succeeds in getting through the whole course of a development depending so greatly on chance. Hence the necessity for such enormous productivity of eggs." (1904, 47).

### CHANCE ELIMINATION

In describing the elimination of the hundred million less one, and the survival or "selection" of the one, it is not surprising that Weismann has to use such terms as the "long chain of chances," "happens to be fortuitously eaten," "if they are fortunate enough" and "happen to be swallowed" in describing a "course of development depending so greatly on chance." The choice of language appropriate to the nature of the process, as I have said, is not surprising. What certainly is amazing, however, is that selectionists can record such matters, and it is to be assumed, reflect upon them, yet cling to the doctrine of "survival of the fittest."

*A priori* that doctrine is incredible anyway when the theoretical survival of only one or two, on the average, out of exceedingly large numbers is concerned. Common sense tells us that survival literally of the fittest can not happen, except by rare chance, but judging from the evidence alone, which as loyal scientists we must do, there is nothing in this world more certain than that the "fittest" do not survive in the cases here detailed of the oak, the orchid, and pine, the cod, the oyster, and tapeworm. Their life histories, moreover, are not unique but may be taken as fair illustrations of those of large groups of organisms with respect to which we must draw similar conclusions.

Recognition of the indiscriminacy of elimination in still another group is given in the following popular science item: Dr. Albert F. Blakeslee estimated the number of spores in a puffball of about 12 x 10 x 9 inches in dimensions to be in the neighborhood of six trillions—enough to allow one spore for every square rod of the entire land surface of the earth. "Theoretically, every one of these spores is capable of giving rise to a new plant of its own kind. Actually, nearly all of them fall into unsuitable places, and so perish." (*Science-Supplement*, 81(2104), April 26, 1935, p. 9.)

### SURVIVAL OF THE ORDINARY

When elimination exceeds 99.99 per cent it must take the fit along with the unfit. Especially is this result certain as in the bulk of the population the margin of superiority of one individual over another is slight, both theoretically and actually. The impossibility of any process, natural or artificial, selecting the fittest from a large number of exceedingly similar individuals makes it certain that in general the survivors will be random samples, or in other words commonplace representatives of their kind.

The great checks, climate and disease, are indiscriminate with reference to many special characteristics of organisms. In fact every check that can be imagined is indiscriminate with respect to some adaptations of a race. Some individuals may be cold resistant, for instance, but not very alert; others may endure heavy parasitism but be relatively poor foragers, etc., etc. "Selection" for one good point may be impossible without simultaneous "selection" of one or more weakness, and "selection" effective under one set of circumstances will not be under others. With checks acting in this manner is it not inevitable that specimens from the common run will be the final residuum?

The force of the argument as to survival of the ordinary can be appreciated by scanning a normal curve of frequency (Fig. 1) which assumably applies as well to fitness as to any other character. The area inclosed by the curve may be taken as a measure of the individuals of the group subject to elimination or "selection." It is conspicuously apparent that the great mass of the group do not vary widely from the mean, and that those which do differ greatly (both the most and the least fit) are relatively few in numbers. The fittest and the least fit are represented by the extreme left and right ends of the figure. Quarters of the population lie to the left and right, respectively, of the quartile ordinates (q, q). The small proportion represented in a less than one per cent survival can readily be envisioned. It is a proportion that can easily be reduced to the vanishing point by any indiscriminate mortality, and as demonstrated in earlier pages, mortality reducing populations at all numerous is indiscriminate. Contemplating the matter with the aid of the graph, it is perfectly evident that a random sample of one or two individuals from the whole population will in the vast majority of cases come from the great median group of rather closely similar individuals. The dotted curve in the graph marks the bounds of the population after experiencing a 50 per cent indiscriminate reduction. In the remainder it is clear, as before, that average and near-average individuals greatly predominate in number over the more aberrant (unfit or fittest).

R. A. Fisher, a modern defender of natural selection theory, may be quoted in agreement with this conclusion, as after remarking that, regardless of genetic constitution, great likeness among members of the same species is necessary even for approximately normal adaptation, he adds, "Since any differences which may exist . . . are certainly extremely minute we have here a clear indication of the closeness with which any tolerably successful individual must approach the specific type." (1930, p. 68).

The totally unfit as well as the most fit do not form very large blocks of the population, so that if all of the former are eliminated and final "selection" made from the average plus the fittest groups, still in all probability survivors are likely to come from the median, and vastly more numerous, group. This type of "selection" is admitted by the ultra-

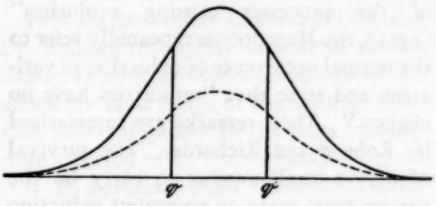


FIG. 1. FREQUENCY CURVE TO SHOW (SOLID LINE) THE NORMAL DISTRIBUTION OF A POPULATION WITH RESPECT TO FITNESS, AND (BROKEN LINE) THE DISTRIBUTION AFTER AN INDISCRIMINATE 50 PERCENT REDUCTION. THE PORTIONS LATERAD OF ORDINATES *q*, *q* ARE EACH 25 PERCENT OF THE TOTAL

selectionist, A. R. Wallace, in the following passage:

"If now we consider the population of a species with regard to any particular character or combination of characters, we may divide it into three groups—a central group in which the mean or average development prevails with little variation, one in which the character is excessively, and one in which it is little developed. These groups would not be of equal extent, the central portion—that in which the mean characteristics prevailed—being, in accordance with the law of averages, much more numerous than the extremes; perhaps twice or even three times as great as either of them, and forming such a series as the following:—Maximum development 10, mean 30, minimum 10. These figures, whatever their exact proportions, would probably be pretty constant, for we have no reason to believe that the mean characters, or the amount of variation of a species, change materially from year to year or from century to

century; and we may therefore look upon the central and most numerous group as presenting the typical form of the species, being that which is best adapted to the conditions in which it has actually to exist, while the extremes, being less perfectly adapted, are continually weeded out by natural selection." (1880, pp. 100–101.)

Wallace probably did not realize that selection of the type he postulates could be no moving force in evolution. The weeding out of variants and the preservation of individuals near the mean is anything but Darwinian natural selection. Nevertheless, it apparently must be recognized as the ordinary occurrence.

In their work on "The relative value of the processes causing evolution" (1921), the Hagedoorns repeatedly refer to the normal occurrence of reduction in variation and state that "minorities have no chance." Their remarks are summarized by Robson and Richards, "The survival of only a small number to carry on the species must mean an enormous reduction in variation each year, probably enough to account for the observed constancy of most species. The chance that any variant represented by only a few individuals will form a part of the next year's initial population is very low, the magnitude of the chance depending (apart from survival value) on the ratio between the numbers of the variant and the total number of individuals in the species." (1936, pp. 171–172.)

Extending the argument, it seems clear that aside from the slight chance, mathematically speaking, of variants in small numbers surviving to propagate their kind, the observed constancy of species proves that they do not, in fact, survive. (The natural selection theory here faces another of its numerous dilemmas; if selection does not restrict variability it cannot direct evolution; if it does restrict variability it destroys the material essential to its own effectiveness.) The type of "selection" (if it may be so called) here referred to is the "periodic selection" of Karl Pearson, defined as reducing variation in each generation without altering the type (1900, p. 413). To put the matter in another way, if the race continues unchanged as in these concepts of Wallace, Pearson, and others, variation must be reduced in each generation and when variation is reduced survivors must necessarily come from the median class.

There are, moreover, unmistakable signs that there is a tendency in nature for the elimination of all aberrant forms, which applies to the "fittest," if these are significantly aberrant, as well as to the unfit. Mutations, as a rule, admittedly are backward steps; usually in fact they are lethal. Is not almost any decided variation likely to be harmful? In this regard W. D. Matthew says, "So with any wide and conspicuous variation from the normal. It is of relatively rare occurrence, and it is a handicap, not an aid to survival." (1930, p. 193). Is not conservatism the rule in nature? We clearly see that it is in behavior, as in the case of lone bulls and rogue elephants; it takes the varying one out of the stream of descent. Despite the universal occurrence of minor fluctuating variations, species nevertheless conform to type. They are so alike as to be recognizable at a glance. Conforming is a universal law (herd law, law of the pack, gang law). It is exactly innovators and innovations that are suppressed, and it seems clear that from Nature's point of view the commonplace are the "fittest."

Out of thousands or millions of ordinary specimens there are hordes of nearly the same orders of abundance that are equally capable of reproducing the race. What is true of large, tends to be true of smaller, numbers, and it is likely that the vast majority of organisms are prolific enough

to invite elimination of much the same character as that affecting their more numerous contemporaries, and that the net result in their case also is survival of fairly typical specimens.

Parents of the new generation coming from any part of the great mass of average or near-average individuals would give rise to offspring which, considering regression and the propensity to vary about a mean, would differ little if any from the preceding generation. Galton from his studies of stature in man "pointed out the remarkable way in which each generation was succeeded by another that proved to be its statistical counterpart." (1886, p. 256.)

There is no evidence that the so-called process of "natural selection" does away with this characteristic of species as the ordinary fluctuating variations appear generation after generation more or less within the same limits and about the same mean. (Lindsey, 1929, p. 268.) This can only mean either that extreme variants ("fittest") are not "selected" or that if selected they do not transmit their divergent characters to their descendants.

There is evidence in specific cases, furthermore, that in nature the range of fluctuating variations is maintained. We quote from Dr. Vernon L. Kellogg, as follows:

> "But aside from the part that what we may call fortune of position plays in determining life or death among individuals, what of the actual rigour of the struggle in those cases where death does not come to thousands at a moment;—in the whale's mouth, by catastrophe of flood or drouth, or by the elephant's tread on the ant-hill? To this question of the rigour of intra-specific struggle I have given some personal attention in insect life, and while to detail observations here would be impossible, I may say baldly that no such rigour of individual selection based on variation in colour, in pattern, in venation and other wing characters, in hairs and in numerous other structural characters, as demanded by the needs of the selection theory, is to be detected. I find just as much variation represented in series of mature individuals collected miscellaneously after having lived for more or less time a free life exposed to all the dangers of this life, exposed, that is, to the rigour of the individual struggle for existence, as among series of similar extent of individuals of the same species collected just at the time of reaching maturity but before enjoying any opportunity to be weeded out (on a basis of disadvantageous variation) by the rigour of the life-struggle. Just as many varying individuals, with variations of just as much extent and variety, were found in series exposed to the struggle, in which these variations are presumably capable of saving or losing life, as among series not yet exposed; in other words, just as much variation exists after enduring the selective rigour of the struggle as existed on the day when the insects are first exposed to it." (1908, pp. 82–83.)

This account refers to coccinellids, and Dr. Kellogg records an instance of similar import relative to *Diabrotica soror* in which about the same range of fluctuating variation was maintained in the same locality over a period of 15 years (1910).

It is apparent from these and other exact records, as well as from ordinary experience, that the population of organic species always shows a more or less characteristic range of variation. This condition in itself is all the proof that is required that extreme individuals ("fittest") are not the chosen propagators of their kind, or that if they are selected, the average character of the species is not thereby affected. It is certain, therefore, that species are maintained along the line of averages (see also Galton, 1886). Variation about a mean, a universal phenomenon, always leaves the bulk of a species at or near the mean. From this mass of closely similar individuals, and not from extreme variants, reproduction proceeds. This is not only a common sense conclusion but is supported by experience and evidence. The burden of proof for any other claim certainly rests upon its makers.

If "selection" of superlative character-

istics were the rule, there would be visibly rapid evolution and a preponderance of bizarre species. The obviously very gradual process of evolution and the equally apparent conservative make-up of species, of themselves, indicate that propagation is from the median class of average individuals and not from aberrant extremes.

It is of interest to quote Dr. E. D. Cope in this connection: "In general, then, it has been the 'golden mean' of character which has presented the most favorable conditions of survival, in the long run." (1896, p. 174.)

In the course of the foregoing discussion of natural elimination and reproduction, two phases of the general subject were mentioned which could not then be more fully considered without interrupting continuity. We will now proceed to these, namely, the disproportionate mortality of the young, and survival by age classes.

### DISPROPORTIONATE MORTALITY OF THE YOUNG

A cause of much mortality in the very young is constitutional defectiveness. The individuals affected can scarcely be said ever to have entered the struggle for existence. They are, so to speak, born outside of it and the cause of their being so born, misfunctioning of the hereditary mechanism, is not a process of "natural selection."

It is evident not only from the examples previously cited but from general biological experience that reduction in organic populations is most drastic in the immature stages. A few further instances may be cited to illustrate the principle. Dr. John D. Harshberger presents (1898) data on the numbers of abortive fruits of various plants from which the following percentages have been computed: *Pimpinella integerrima*, 22.65 per cent; *Hibiscus moscheutos*, 25.68 per cent; *Cornus florida*, 74.62 per cent; *Arisaema triphyllum*, 78.85 per cent; and *Azalea nudiflora*, 95.74 per cent. These, bear in mind, are losses to a more immature stage than the perfect seeds or fruits and later the seedlings, which themselves are subject to wholesale destruction.

Losses in the seedling stage have recently been emphasized by Professor E. J. Salisbury. He cites reduction in the number of seedlings of *Silene conica* from 175 to 9 per square decimeter in the period from September to January, and comments: "There is not only a very high mortality amounting to nearly 95 per cent, but also the important fact is that this mortality was entirely confined to the seedling stages. All the mortality occurred prior to the formation of the second pair of foliage leaves, and all the nine survivors will clearly attain the flowering condition."

Relating to *Verbascum thapsus*, he says: Of "several hundred thousand seedlings" all but 108 died during the first six months. "All the survivors which attained the rosette stage flowered and produced seeds."

Similar occurrences were noted also for *Ranunculus parviflorus*, *Helleborus viridis*, *Cochlearia danica*, *Dianthus prolifer*, and *Fagus sylvatica*. The author therefore justly concludes that "in these plants, at least, the mortality and therefore the operation of natural selection is almost entirely confined to the juvenile stages of development." (1930).

Thus elimination in the immature stages which we have shown to be highly indiscriminate, was in the cases Salisbury describes, the total elimination. All survivors of that random selection matured or clearly promised to do so. In other words all were fit, a result not at all compatible with the alleged "survival of the fittest."

The same type of mortality prevails

among animals and its relation to "survival of the fittest" theory is the same. The entomologist, F. Muir, in writing of biological control says, "My own experience covering such work has demonstrated to me that in the majority of instances the greater part of the mortality falls upon the eggs and younger stages, so that only a very small proportion comes under the influence of selection, so far as the adult characters are concerned." (1931, pp. 3–4.)

In combating Darwin's hypothesis of sexual selection, Wallace used the argument that selection in the adult stage, alone, would be ineffective, and if the argument is valid in that case, it holds good in the entire field of natural selection. Wallace's remarks are: "In butterflies the weeding out by natural selection takes place to an enormous extent in the egg, larva, and pupa states; and perhaps not more than one in a hundred of the eggs laid produces a perfect insect which lives to breed. Here, then, the impotence of female selection, if it exist, must be complete; for, unless the most brilliantly coloured males are those which produce the best protected eggs, larvae, and pupae, and unless the particular eggs, larvae, and pupae, which are able to survive, are those which produce the most brilliantly coloured butterflies, any choice the female might make must be completely swamped." (1923, p. 296.)

If natural selection is to operate in the manner usually claimed, it must be upon individuals that have not completed their reproductive cycle, in most cases therefore upon the immature. But the immature in general do not have the characters that are said to insure the survival of the species, hence "selection" among them cannot directly control the perpetuation of those characters; this point has been commented on also by Henslow (1893, p. 333), Salisbury (*op. cit.*), and Willis (1907, pp. 17, 18 and 1922, p. 210).

The heaviest losses are suffered by the immature stages, but this is not only because the young are relatively helpless but also because they are most numerous and under the principle of proportional predation (McAtee, 1932, p. 144) must bear the brunt of assaults by natural enemies. There are more of them, they must serve, just as grass is eaten by more terrestrial organisms than are other plants.

The trend of the examples more fully discussed in earlier pages of the present paper certainly is to the same effect as that of the Salisbury cases, namely, that mortality is so heavy in the immature stages that there is very little scope for additional "selection" in cutting down the number of adults to the average limit of the breeding population. The survivors are a uniform or standardized lot of average type and there being no superlatives among them there is no opportunity for "selection" of the "fittest." There may be those who believe that the requirements of evolutionary theory will be met by a small degree of "selection" among the residue that have survived the hazards of immaturity, but they cling to a faint hope and have no excuse for longer indulging in that full-blown speculation, termed "Survival of the Fittest."

We scarcely need repeat that the evidence is preponderantly to the effect that mortality among the immature stages is almost entirely ruled by chance. The survivors, therefore, will be a random lot, containing all degrees of fitness normal to the species. Since numerical reduction of the species already has about run its course most of these survivors will reproduce. Even if there were selective mortality in the immature stages, it could have little more than a general bearing on adult fitness because of the differences in the

characters involved. Willis remarks concerning plants, "while the characters that distinguish species and genera are largely characters of the floral organs, the struggle for existence is almost entirely among the seedlings and young plants, in which these organs are not yet present." (1907, p. 17.) As a special case consider the numerous metamorphosing animals in which immature stages are utterly different from, and in many cases even inhabit a different medium from, the adults. Moreover the period of adult life in such organisms often is a relatively brief one allowing little time for "selection" to act. Yet if "natural selection" is to explain evolution of species, it must be through "selection" of adults, the breeders of future generations. In many cases, it is certain that mortality among the immature is so sweeping that it leaves opportunity for little reduction in numbers and hence for "selection" among adults.

To recapitulate the few preceding paragraphs: Mortality among the immature stages is both sweeping and indiscriminate; practically all of the survivors, a normally variable, not a selected, lot reach maturity and may propagate; and although evolution must go on through the medium of adults, only a small proportion of the total elimination (or "natural selection") occurs in that stage. The chances seem poor indeed that there is any such thing as "survival of the fittest."

### SURVIVAL BY AGE CLASSES

Survival by age classes was incidentally mentioned in the accounts previousy given of the western yellow pine, the codfish, and the Atlantic oyster. To emphasize the significance of these cases the following expanded discussion is given.

Mr. G. A. Pearson, the author quoted with regard to the pine, not only recorded the usual high mortality to seeds, but also the phenomenon of "seed years," one of considerable interest for its bearing on selection theories.

"It is believed," he says, "that the long period between good seed crops may be due to circumstances not connected with the fruiting habits of the tree. Repeated observations have shown that enormous numbers of cones die during the first season. . . . It is possible that late frosts have more to do with these conditions than dry winds. Whatever may be the cause it is probable that many of the light seed crops would be good crops but for the arrested development of cones in the early stages. Much damage is done by weevils as the cones approach maturity, but serious infestations are usually of a local character." (1923, 21–22).

"During the period covered by the rainfall graphs, good seed crops occurred in 1908, 1909, 1913, and 1918. Since the seed does not germinate until the year after it is borne, the right coincidence between seed production and rainfall occurs only when a seed year immediately precedes a period of ample moisture. In 1908 and 1909 seed crops both failed to give results because they were not followed by sufficiently long periods of immunity from severe drought. Similarly, the favorable moisture periods of 1911–12 and 1918–19 were unavailing because they were not preceded by good seed crops. The seed crop of 1913 was effective because no really serious drought occurred until the foresummer of 1916. It would have been still more effective if there had been more precipitation during the summer and fall of 1915 or if the seed had been available a year earlier, thus taking advantage of the entire rainy period, which extended from July, 1913, to August, 1915. The grand climax came with the heavy seed crop of 1918 and the extraordinary precipitation of 1919, followed by reasonably favorable moisture conditions in 1920. This period promises to give the best reproduction experienced on the Coconino in 40 years. Success would have been still more certain, however, if the seed which matured in 1918 had been available a year earlier, thus profiting by the two consecutive wet summers of 1918 and 1919. During the entire period of 12 years, two good seedling crops of general distribution have originated on the Coconino and Tusayan Forests, and only one of these, that of 1919, was adequate to give full stocking over extensive areas." (*ibid.* 28–29).

Commenting further on this matter, E. N. Munns, Chief, Division of Silvics, U. S. Forest Service, writes (letter of March 27, 1933),

"Foresters are coming to believe that factors other than periodicity account for seed production and that the forest tree fruits are susceptible to damage from early fall frosts while the buds are being formed, spring frosts as the flower buds start to develop, fog or rain at the time the pollen is ready to be liberated, excessive drying in the early spring, and other such climatic troubles.

"There has been no real crop of Douglas fir seed in the Pacific Northwest for a period of 10 years. Investigations disclosed that flowers were borne practically every year; some one or several of the climatic factors indicated have been responsible for the failure of these flowers to develop.

"In the Southwest the seed periodicity of *ponderosa* pine is quite marked. However, the seed does not catch and reproduction does not get started unless there is a coincidence of seed year with favorable climatic conditions. This has given rise to even-aged groups of trees extending in some places over considerable areas. Investigations have shown that the coincidence of good seed years and climatic years favorable for reproduction occur at intervals of from 15 to 40 years."

The survival of even-aged groups of trees is not a phenomenon confined to conifers nor to the western part of the country. In the vicinity of Washington, D. C., it is evident that the Virginia pine "catches" by seed years, and the beech which very rarely fruits here is as common as it is in some regions where it matures nuts regularly.

In the case of the codfish we are informed by a letter from the U. S. Bureau of Fisheries (March 17, 1933) that "there is almost a total failure of the natural hatch during certain years, while in other years the species is very successful in establishing an abundant generation. We thus find that in the commercial catch . . . certain year classes greatly predominate." In other words "fitness" in the case of the cod means being hatched in the right year. In one year few or none are fit to survive, while in another a high proportion are. Does this not put the theory of survival of the fittest in a sufficiently ridiculous light? Can it with any semblance of reason be claimed that the fittest of the cod eggs survive? Certainly not; in the favorable years a high proportion mature, in the unfavorable seasons, from unquestionably just as good foundation stock, few or none survive. Not fitness of the cod but that of the environment rules.

The same survival by year classes is known in the case of mackerel, herring, salmon, and other fishes, and it may be taken as axiomatic that species producing large numbers of eggs are subject to powerful devastating influences, the action of which is little affected by fitness. In the case of the extraordinarily prolific Atlantic oyster, we are informed that "very good spatting seasons occur sporadically. . . . In the same locality bad and moderate seasons have occurred on the average about twice as frequently as good or very good seasons. Similar fluctuations occur in all parts of the world. . . . In times of scarcity when the spatfall has failed for a longer succession of years than usual, and when the adults on the beds have been reduced beyond a certain minimum, particular beds have gradually died out." (Orton, 1929, 1004).

Bad years twice as numerous as good years, spatfall failing for a succession of years—the nature of the case is the same as in those of the pine and the cod. Sometimes complete failure, sometimes great success, but at all times upon the same basis of potentially uniform reproduction from which "nature" is supposed to, but obviously does not, "select" the "fittest."

According to the natural selection theory only two on the average out of any number of offspring of bisexual species are fit enough to survive. Survival of large numbers by age classes completely demolishes this pretense; a high proportion prove fit. The converse of this proposition, namely, that in the years of failure

due to environmental conditions, none are fit, although reproductive effort is just as strong, and the quality of the progeny just as high, is, of course completely absurd. The sweeping failures demonstrate that all the eliminated are not unfit, while successful mass reproduction shows that it is not only the "fittest" that survive. Mass survival must result in a preponderance of ordinary or typical, not aberrant ("fittest") representatives of species.

### SURVIVAL AMONG CYCLIC OR FLUCTUATING SPECIES

Similar conclusions must be drawn from the ups and downs of cyclic species. At the height of their abundance they are swiftly cut down to minimum numbers; according to the "natural selection" test nearly 100 per cent are unfit. Then begins an upswing in numbers, during which the rate of survival is so high that the species soon regains its abundance; i.e., a high proportion are fit. The impossibility of explaining these results by the theory of "survival of the fittest" is manifest. Aside from the great improbability that it is the "fittest" that survive the "drops," consider what they do to prove their "fitness." They propagate a population that in a few years again is swept away in exactly the same fashion as their own generation. Better proof could not be asked that the "selected" breeds are merely ordinary members of their race.

The cycles of the snowshoe hare, for instance, are known to have been going on for a century, and the great probability that they have an underlying climatic cause indicates that they may have occurred for an indefinite period in the past. In these cycles at the period of the "drop," "crash," or "die-off," the "selection" is the most drastic imaginable, apparently just short of extermination. If "survival of the fittest" had prevailed through the ages, the cyclic races should have developed some sort of resistance to the forces that destroy them. The fact that they remain subject to extreme fluctuations is sufficient evidence that there is no "survival of the fittest" or race improvement; if there were the cycles would end. Similar conclusions apply in the case of any species that undergoes great fluctuations in numbers, and the indications are that a very high percentage of all existing species are so affected. For various reasons, some of which at times we think we can descry, a species becomes relatively rare; its "fittest" are few indeed. Then its ranks are rather rapidly refilled by the survival of a much higher than usual proportion of young; its "fittest" for the time being are numerous. The same remarks as to the average character of survivors, are in order, as in the case of cyclic species. "Natural selection," the "survival of the fittest," do not explain such phenomena and they are practically universal.

### CONCLUSION

Among large numbers of organisms, the "fittest" (two out of millions for instance) are an exceedingly small proportion. It is wholly incredible that any process could be devised even by man, much less brought about by the forces of nature, that could insure "survival of the fittest."

It is evident, moreover, from the instances discussed in this paper that elimination, which bears most heavily on the immature stages, tends to be highly indiscriminate. For this reason also it is very probable that, as a rule, not "fittest," but commonplace specimens survive to propagate the race.

Elimination among the immature being

practically indiscriminate, the survivors will be a random or ordinary lot. Since a high proportion of the total elimination occurs among the young, there is opportunity for comparatively little "selection" later among the adults, and most of them (necessarily of average make-up) will breed.

Many species have a tendency toward an "all or none" type of survival. It is very strongly developed in those exhibiting age-class phenomena, and is only slightly less evident in cyclic species and others subject to considerable fluctuation in numbers. The mass elimination these species experience (sometimes total for certain periods or areas) certainly takes the "fittest" along with the rest, while their characteristic mass survival obviously is not limited to the "fittest" (a group which according to selection theories, as we have already noted) is very small. There is no conclusion possible, therefore, but that the continuity of these forms (which include probably a great majority of all existing species) is through ordinary representatives. On all these counts (elaborated in previous pages), therefore, the unavoidable deduction is that average representatives of species, not the "fittest," survive.

What do these considerations mean in relation to the theory of "natural selection?" The preservation of the commonplace or fit is something very different from the "survival of the fittest." The latter, though incredible, if it existed, could produce evolution; the former, and it is certain, what actually occurs, by itself cannot.

"The survival of the fit," "the elimination of the unfit," favorite succedanea of neo-Darwinians (of themselves) will not do the business, will provide no motor for the evolutionary machine. Even "the better equipped survive, the worse equipped die," idea is scarcely an improvement, because of the reversion that must always occur to account for the observed maintenance of fluctuating variations. From better or "fitter" to fit or ordinary is a gradation so small that it is easily within the scope of the usual range of variation. It is certain that descendants of organisms so characterized would, on account of merely normal regression and variation, be indistinguishable from the average of their kind.

The evidence marshalled in this paper is to the effect that reproduction of species, on the whole, is carried on by ordinary individuals. They come from the great median mass of the population, and maintain the normal range of fluctuating variations. In the absence of other effective forces, propagation in this manner cannot change the character of the race nor produce evolution.

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# THE CYCLIC CHARACTER OF HIBERNATION IN FROGS

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## INTRODUCTION

HIBERNATION is usually thought of as a resting condition in which an animal exists in a state of complete or partial torpidity. This state is associated with low temperature, but, a similar condition, called aestivation is found at high temperature and low humidity.

Many causes have been suggested to account for this torpidity, but, whatever the cause may be, the result seems to be that the animal is able to live through unfavorable weather conditions with no lasting injury to itself. This period of inactivity in a protected location offers the opportunity for carrying through the physiological changes in the life-cycle.

Six different modes of expression of the tendency to become quiescent occur in the animal kingdom. The extreme specialization of the reaction to unfavorable conditions is to be found in some protozoans, rotifers, and copepods, where the animal loses its normal shape, and many of its physiological characteristics are modified so that, in a new form, the cyst, it is able to withstand great extremes of temperature and moisture.

Insects may be used as examples of the second type, for the animals become more or less dormant regardless of environmental factors. The codling moth prepares for hibernation by heavy feeding, which is followed by the elimination of all waste, then burrows into the ground and hibernates. If they cannot burrow, they remain active and cease to feed for several weeks (Tower, 1906). Shelford (1908) found that tiger beetles become dormant in August and continue so all the winter with no apparent environmental factor responsible for bringing on quiesence.

A third type comprises invertebrates that develop accessory protective devices against unfavorable weather. For example, in a dry soil annelids may be found knotted together in a cyst-like ball and covered with mucus while awaiting increased moisture. Another example is found in the terrestrial snails which with the approach of unfavorable weather withdraw into their shells secreting successive mucus diaphragms across the apertures. With this group may also be classed the terrestrial reptiles and insects that seek shelter from unfavorable conditions without passing through any evident physiological change.

A fourth type represents true dormancy and may be found in homothermic mammals, such as the marmot, which assumes a state of poikilothermism as an adaptation against unfavorable environment. The fifth type is found in the bear which observers assert, merely "dens up" to maintain a high temperature when food becomes scarce. The sixth type embraces certain frogs and other poikilotherms which enter lethargy without the necessity for a change in the body temperature regulation.

There is a fairly large body of literature dealing with the nature and causes of hibernation, but little of it deals with

the amphibia. The conditions in insects as well as in mammals have received attention, and there are several extensive summaries of the literature on these groups with very complete bibliographies. Dubois in his "Physiologie Comparé de la Marmotte" reviewed the literature to 1896, and Polimanti (1912–14) and Rasmussen (1916) discussed the literature to 1916. Pocock summarized the work from the standpoint of general biology, while Gorer (1931) studied the subject from the angle of comparative physiology. Particular mention should be made of the work of Johnson (1931) and also an excellent compilation contained in the monograph of the German writers von Ferdmann and Feinschmidt (1932).

It is the purpose of this paper to summarize the literature for the amphibia, and show that hibernation is a phase of the annual cycle of the common frog. This cycle is evident in the changes of weight and of longevity under unfavorable conditions, and in seasonal differences in the glandular portion of the hypophysis, gonads, adrenals, pancreas, thymus, thyroid, spleen, chromatophores, muscles, heart, and blood.

It will be seen that this study together with that of Johnson (this journal 1931) brings the knowledge of hibernating vertebrates up to a late date.

There is so little literature on the factors concerned with the hibernation in amphibia that the author has been compelled, for the purpose of comparison, to include some of the more important contributions on the hibernation of other forms. The recognition of the nature of the differing behaviour of the animals is based upon the interpretation of physiological and other studies. Much of the older work on the frog has been presented without due consideration of the time of the year during which the observations were made; this has led to a multiplicity of inaccurate and contradictory statements.

### SUGGESTED CAUSES FOR HIBERNATION

(*A*) *Temperature*. The cause of lethargy has been commonly ascribed to the factor of environmental temperature. Lethargy was held to be the result of stiffening of muscles as a reaction to the cold (Spallanzani, 1792), while in 1926, Adler held that cold was a factor in causing hibernation. The heat produced by the animal was not sufficient to cope with the environment, the blood became cold and the animal torpid (Buffon & Lacepede, 1920). On the contrary, studies of marmots have not shown that cold is the cause (Mangili, 1807, Mares, 1892, Cleghorn, 1910, Rasmussen, 1916). Mangili even claimed that severe cold arouses hibernating animals. Nor do the studies on insects point conclusively to temperature as a factor.

Since hibernation in amphibia has been studied but little, it was thought that a study of *Rana pipiens*, subjected to zero degree centigrade at each season of the year would be instructive, and accordingly during each month, throughout a period of two years, groups of *Rana pipiens* were put into an electric refrigerator and kept at zero centigrade temperature and saturated humidity. Only those frogs which were placed in the refrigerator from October to April became completely torpid. They buried themselves in sand in a position characteristic of the hibernating animal, i.e. with arms folded under the chin, the hind leg drawn close to the body and head resting upon the front legs. The eyesockets were drawn into the head. In the summer these frogs became sluggish, but at no time did the cold cause complete torpidity. These results lead to the conclusion that external

temperature is of itself not sufficient to cause hibernation in *Rana pipiens*. However it was shown in these investigations that external temperature is a necessary factor in the development of physiological conditions attending dormancy as was demonstrated in the increase of weight during the months of winter.

(*B*) *Food*. A lack of food is also an environmental factor in hibernation, and has been believed to make the animals more susceptible to cold (Hall, 1832, Mangili, 1908). Others have observed that marmots will hibernate with food available. Accumulation of fat was proposed as a cause of lethargy by Claparède (1905). In nature frogs bury themselves in the mud in October and remain without food until March (Barthelemy, 1926). No food was available to these frogs in the refrigerator at any time of the year, yet in the months of winter the frogs became lethargic and in the summer grew merely sluggish in response to the same environment.

It is of interest here, to note that the "control-frogs" which lived in atmospheric temperature did not attempt to snap at insects placed in the cages during the winter months. Autopsy showed no insects in the contents of the stomach, from October to February, although beetles and worms were placed in their cages.

(*C*) *Moisture*. An environmental factor suggested as a cause of hibernation, is a lack of water. Desiccation of vegetables has been thought to cause lethargy in marmots (Kasharov, 1907, Shaw, 1925, Kalaboukhov, 1920). Contact with water is said to be one of the controlling factors in the emergence of certain Orthoptera (Bodine, 1921, Fink, 1925). Likewise, emergence of toads and salamanders has been attributed to rain (Townsend, 1921, Blanchard, 1930).

In this work on *Rana pipiens*, moisture could not be considered a factor in determining emergence since, throughout the entire period of study, the humidity in the refrigerator was constantly saturated. The melting ice on the shelf above the frogs, kept the sand in which the animals hibernated, constantly moist, and still, without any change in the distribution or content of the moisture in the refrigerator, the hibernating frogs became active and tried to climb out of the box when the door was opened. During hibernation there was a hydration of the animals, but upon emergence hydration decreased.

### THE INTRINSIC FACTORS

(*A*) *Nervous System*. An essential characteristic of the hibernating animal is its ability to change from a homothermic to a poikilothermic type at a rather definite period. In the hibernating mammal the heat center apparently fails to function and the animal reverts to a condition of the animal with a more primitive type of nervous system (Barkow, 1845, Pembrey, 1895, Noe, 1903, Merzbacher, 1904). If the caloric center is known a satisfactory answer is still lacking as to how it is possible in so short a time for hibernants to cause this center to become active or inactive. What are the stimuli for increased or decreased activity? Certain animals roll themselves into a ball while hibernating, owing to tonic stimulation of adductor and flexor muscles. The center for the clinging reflex shown by bats in hibernation is thought to be in the medulla.

Diminution of Nissl's granules and a general basophile reaction of the cytoplasm, have been observed in the hibernating mammal (Legge, 1899). In the present experiments it was found that the hibernating frog assumes a position in which the outside curvature of the body

is a segment of a circle. When a dormant frog is electrically stimulated in any manner, the circle tightens, but when stimulation is increased, a series of random jumping movements occur.

A structural relationship between hibernation and the nervous system in frogs has been found also (Donaldson, 1911). Levi (1898) found seasonal alterations in the staining reactions of the cord of a frog. The author found a seasonal difference in the staining reactions of both cord and brain, as well as of other tissues. The nuclei and the cytoplasm of the frogs were less basophilic during the winter. In the hibernating frogs the cytoplasm loses its basophilic character entirely. Donaldson (1911) found that the relative weight of the central nervous system in the leopard frog was low at the time of hibernation.

Seasonal changes were observed in the functioning of the nerves of *Rana pipiens*. Stimulation of the vagus failed to stop the beat of the heart in hibernating frogs, while the same stimulation inhibited the heart of active frogs of the summer. An alteration in nervous control is indicated by a change in the absorptive power of the skin of dormant frogs. Adolph (1921) found that the nervous system is responsible for resistence to osmosis. As will be discussed later, the frog absorbs water during dormancy. Undoubtedly the nervous system plays an important part in the development, duration and awakening from hibernation, but apparently as a phase of the seasonal cycle only.

(*B*) *Endocrine Control*. The possibility of a relationship between the secretions of the endocrine system and dormancy has been the cause of much speculation.

*Glandular Portion of the Hypophysis*

Observations on the pituitary are not in agreement as relating to hibernation. In mammals the gland diminishes in size, and the cells in the pars-anterior completely lose their characteristic staining reactions to acid and basic dyes (Cushing & Goetsch, 1916). These observations were corroborated by the experimental work of Coninx-Girardet (1927), who found that a decreased activity in winter was very definitely paralleled by the reduction in the number of basophilic cells and a more open grouping of the cells of the pituitary. The histological conditions during spring and summer indicated increased activity for the breeding periods. On the other hand, Mann (1916), Rasmussen (1921), Bugbee, Simon and Grimes (1931), were all unable to discover any change in the hypophysis which was produced by hibernation. Johnson and Hannawalt (1930), injected pituitrin in doses of two hundred and twenty-two to four hundred and fifty times the human dose, on the basis of relative weight and found that in sixty-six of the one hundred and fifty marmots, it produced no change in the tendency to hibernate. They concluded that the secretion of the posterior lobe of the pituitary is not an important agent in production or prevention of dormancy.

Observations on the glandular portion of the hypophysis of *Rana pipiens*, made by the writer, showed marked changes between the tissues of the frogs in hibernation during the winter and those of the summer season. The nuclei were angular in shape and much larger, with fewer nucleoli during hibernation than in the active animals of summer. The nuclei as well as the cytoplasm of the dormant frogs were less basophile.

*The Gonads*

According to Oslund (1928), there is a seasonal variation in the testes and also in the relative quantity of constituent

elements in most animals that breed once each year. In the hibernating animal the change appears to be more pronounced than in the non-hibernating forms. Gonadal activity in general, tends to prevent hibernation in marmots in the spring, but not at other seasons of the year. Castrated animals hibernate to a significantly greater degree during the breeding season than do the normal individuals (Mann, 1916, Rasmussen, 1917–18, Drips, 1919, Shaw, 1926, Johnson, 1930).

In some hibernating animals the seasonal cycle of the testes reveal striking changes in the tissues of these organs (Moore, 1926). Hanseman (1902) observed that the hibernating marmot had only slight amounts of interstitial tissue between the seminiferous tubules. Typical Leydig cells were lacking and spermatogenesis was in abeyance. In the height of the breeding season, two months after the period of awakening, the Leydig cells were so large and numerous as to give the testes the superficial appearance of a large sarcoma. According to Ganfini (1903), the Leydig cells were not absent from the testes but were smaller during hibernation. Regaud (1900) found that active spermatogenesis in a mole was not accompanied by hypertrophy, for following the breeding season the interstitial cells became maximal and remained so after the disappearance of the generative portion. Two hibernating mammals may be entirely opposite with respect to the increase in Leydig cells, and decrease in the reproductive activity (Moore, 1926).

Gonadal hypertorphy has been found in toads and frogs during hibernation (Freidmann, 1898, Champy, 1913–24, Aron, 1921–24). In 1926, Barthelemy placed *Rana fusca*, in artificial surroundings and found sexual maturation, if kept at from 2–3°C., but, at 8–10°C., they remained active and did not mature sexually. He states that hibernation cannot be kept up indefinitely even if the temperature remains low, because it is ended by sexual maturation.

The author's findings in these studies on *Rana pipiens*, are comparable to those of *Rana clamitans* by Aron (1921), and *Rana fusca* by Barthelemy (1929). The frogs of November to March inclusive, all showed development of the secondary sexual characters. A very well defined seasonal cycle was seen in respect to variations in color of the testes. They were white in the summer and deep yellow in the winter. The frogs of March and April exhibited deep yellow or orange coloration in the testes; in May twenty-five per cent were deep yellow and twenty-five pale yellow, while in July the color was six per cent yellow and in August all of them were almost white.

Force (1933) noting this same difference in the color of the testes, attributed it to the age of the individual. She described the testes of fresh specimens as showing the deepening of color, with white for the younger frogs, and a range from white to deep yellow or orange as an indication of the age of the individual. Her observations agree with ours, but her interpretation differs from our conclusions, that the yellow testes indicate an animal that has reached maturity before the breeding season. Our interpretation is based upon the fact that two hundred frogs of a uniform size, displayed a definite range of color from white to yellow, but that the height of the yellow coloration came just before the breeding periods and the white just after. Close observation proved that in August some of the larger frogs of these experiments had white testes, while in March animals which were smaller in size had yellow organs.

In the histological, as well as the gross

structure of the testes of frogs placed in the refrigerator in winter, there was a marked difference from that shown in the animals placed in the cold in summer. Although the frogs were lethargic, the development of the gonads continued up to the time of awakening from hibernation. Frogs became active in March or April regardless of the fact that the temperature in the refrigerator remained exactly the same. The testes of the male frogs contained many sperm, and the coelome of the female was filled with eggs. Controls which were kept at room-temperature showed no enlargement of the gonads. The testes remained white in color and small in size, with very few spermatozoa. Ovaries contained very small eggs. In the event that frogs were not removed from the refrigerator immediately upon awakening from hibernation, they remained active for twenty-four hours, when death ensued.

*The Adrenals*

In mammals there is an enlargement of the adrenals in the spring, but very little change occurs in the winter, except in the blood vessels (Mann, 1916). The span of life for adrenalectomized marmots of the summer is from one to ten days, while the operated animals in winter, survive the normal period of hibernation in apparent health (Britton, 1931). Likewise, winter-frogs survive ablation for one or two weeks, whereas the summer-frogs die in as many days.

A definite cycle was observed in the gross appearance of the adrenal glands of the frogs throughout the year. This cycle was noted in the color of the bodies, in the staining reactions, in the character of the nuclei and in the varying presence of acidophilic granules. The glands of *Rana pipiens* and *Rana catesbeiana*, during hibernation and the breeding period, were deep yellow in color, with a beaded appearance. The glands of the frogs in June, July and August were merely inconspicuous red spots deeply implanted in the surface of the kidneys. By September the color became white or pale yellow, growing darker through October, November and December, reaching a deep yellow or orange in January and February, remaining thus until the breeding season.

In sections stained with haematoxylin and eosin, the cells of the cortex and medulla appeared to be more or less intermixed. The part corresponding to the cortex, consists of polyhedral cells, and those of the medulla were irregular and interspaced with large capillary blood-spaces. The most noticeable difference in the tissues of the adrenal glands of the dormant and active frogs is perhaps the variations in the affinity for basic dyes. In the hibernating frog, the nucleus and the minutely dispersed chromatin, as well as the cytoplasm were slightly basophile. Cytoplasmic granulations were seen, but they stained indifferently.

Acidophilic granules were found in the adrenal tissues of the active frogs. Another difference in the adrenal tissues of the active individuals was manifested in the shape of the nuclei. In the active frog, they were larger and round in shape, while in the dormant animal they were flat or triangular.

*The Pancreas*

Artificial hibernation has been produced in marmots by the injection of sufficient insulin into the blood stream to cause profound hyperglycemia. When this is done, the animal loses its power of control over temperature and if placed in a moderately cool environment, passes into a state of artificial hibernation, the characteristics of which are poikilothermism, a loss of consciousness, a lowered metabolism and

a loss of sensibility to painful stimulation. This state of torpor may be prolonged by the administration of insulin at regular intervals. The injection of glucose terminates the condition; as the temperature rises, shivering begins and the animal returns to normal (Dworkin and Finney, 1927). According to Dische, Fleishmann and Travani (1931) the production of artificial hibernation through over-doses of insulin, requires a higher degree of hyperglycemia than is encountered in the animal hibernating naturally.

Aron (1925) found the structure of the island cells of frogs in autumn and winter to differ from those of spring and summer. The island cells of the dormant animal showed repressed functioning, while the animals of spring and summer manifested activity. Cells of the hibernating type were totally absent in summer, though a few of the summer type are always present.

The gross structure of the pancreas gave little evidence of a seasonal difference between hibernating and active individuals. The color of the pancreas in a dormant frog is white as compared with that of the active animal, which is pink; this variation is due to the presence of erythrocytes in the glands of active frogs. Microscopically, a difference was shown in the cells of hibernating and active frogs. In winter, the cells were large and rich in cytoplasm, which had little affinity for haematoxylin. The nuclei were flattened or perhaps triangular and were not basophilic.

In the pancreas of the active frogs, the nuclei of the cells were round, containing many nucleoli. Around the nuclei of certain cells were many acidophilic granules. The cytoplasm was quite basophilic, in general, giving a darker aspect. Actual measurements of the size of the cells and nuclei were not made, but the microscopic examinations indicated that the nuclei as well as the cytoplasm of the cells are larger in the hibernating frog. The difference shown in the appearance of the pancreas of hibernating and active frogs could not have been due to errors of technique, for the tissues were similarly fixed, mounted on the same slide and stained together.

In the active frog, the islands appeared distinctly paler than did the surrounding cells, but in the dormant individuals all tissues were equally devoid of stain. No secretory granules were observed in any of the island cells.

### *The Thymus*

In literature on mammals, no reference was found pertaining to the thymus as relates to a cause of hibernation. However, in the frog the thymus presents the most striking seasonal cycle of all the tissues studied. In the external appearance a seasonal cycle was observed. This gland is quite large in dormant animals and is of a deep orange color. It has very little blood in the veins or arteries. As frogs reach the end of the period of dormancy the color changes to a pale yellow, diminishes in size, and dissection is attended by much bleeding.

In the active frog the condition is quite the reverse, for the reason that the gland is quite small and so well supplied with blood that superficially it appears to be one large clot of blood.

Frozen sections of the thymus of a hibernating frog showed that the cells contained a yellow substance which resembled fat. These globules appeared unstable when freed from the lymphoid reticulum and varied in size, from a few large, to many very small ones. Routine histological technique dissolved out the lipoid material. (See Figures 1 and 2.)

Figure 1 shows a section of the thymus of a hibernating frog which was killed

February 27, 1934. In some parts of this tissue the cells contain only a large globule which squeezes the nucleus to the side, as in ordinary fat cells, while in other portions, the tissues are made up of cells containing numerous small globules which are grouped around a central nucleus. These cells are polygonal rather than round. Figure 2 shows a section of thymus gland from a frog which was kept in the cold and killed in July. The cells were not vacuolated and contained basophilic granules.

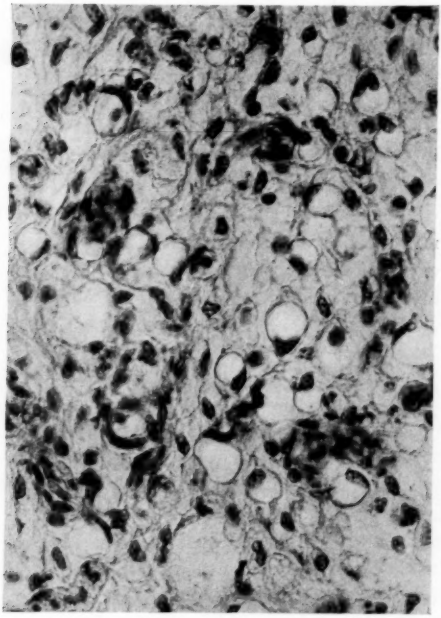


FIGURE 1. SECTIONS OF THYMUS GLAND OF A HIBERNATING FROG

Magnification 300. Frog was killed February 27, 1934 and tissues were fixed in Bouin's fluid and stained with haematoxylin and eosin.

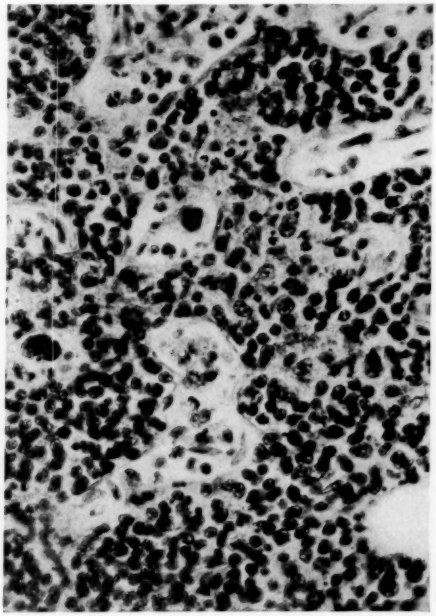


FIGURE 2. SECTION OF THYMUS GLAND OF FROG KEPT IN THE REFRIGERATOR AND KILLED IN JULY

Magnification 300. Fixed and stained as in Figure 1.

### *The Thyroid*

No histological evidence has been adduced to prove that the thyroid has a part in the initiation of dormancy, or, in causing the awakening (Mann, 1916, Johnson and Hanavolt, 1926, Gorer, 1930), but it appears that the increased activity of the gland in the spring accompanies the awakening (Coninx-Girardet, 1927). Histological changes have been found in the thyroid of bats and hedgehogs, that would indicate excessive secretion of colloid just after awakening in the spring. In the fall there is a diminishing of colloid and a flattening of the cells (Adler, 1920–27, Schenk, 1922).

Sklower (1926) found that during adult life in frogs, the thyroid undergoes certain cyclic changes through the storing of colloid in winter and releasing it in the summer. A seasonal cycle in the gross anatomy of the thyroid glands of the female frogs of *Rana pipiens* was observed, in that this gland was larger at

the breeding periods, and smaller during hibernation of the animal. In the hibernating frog the thyroid gland of the female exhibited a deep yellow color, though the male showed less coloration. Glands observed in the non-hibernating frogs appeared to be red, due to the fact that there was much blood in the tissues. The thyroid gland of the hibernating frog was definitely inconspicuous and all attempts to dissect it in the active animals were attended by profuse bleeding.

### *The Hibernating Gland*

Reference to a structure, called the "Hibernating Gland," appears often in the older literature, as a cause of hibernation. The species in which this gland has been found, its location in the body and the authority therefore, are to be found in a table originally compiled by Auerbach (1902), added to by Polimanti (1902) and finally extended by Rasmussen (1923).

This glandular type of adipose tissue was described as being functionally related to the endocrine glands, especially the thyroid and adrenals, and in order of their importance, the regions most often mentioned as a site of the gland, are the thoracic, cervical, axillary, scapular, renal and inguinal (Cramer, 1920). No reference of such a structure in frogs could be found in the literature, but an interesting similarity was observed by the author, between the structure as described for the so-called hibernating gland, and the thymus of the frog.

### *The Spleen*

Mann & Drips (1917) made the statement that within twelve hours after a mammal became torpid, the spleen was found to be greatly enlarged and much darker in color. Microscopically, the organ presented a most intense congestion, but the removal of the spleen was found to have no effect on hibernation.

A similar condition was observed in this work. Besides congestion there was apparently little change in the spleen, though a seasonal difference was strongly manifested in the size and color. In approximately two-thirds of the individuals studied, the spleen of the hibernating frog was found to be from two to three times the size of that exhibited by the active animal, and the color was purple, while that of the active frog was usually reddish brown.

Jordan and Speidel (1925) studied the spleen of the frog in the spring after hibernation, when the marrow in the long bones is hemopoietically active. During this period the spleen showed little activity, and appeared to be merely a reservoir for the blood-cells.

In general, the tissues of all the ductless glands studied revealed characteristics in common, for the seasons of the year. In the hibernating frogs the nucleus contained much less chromatin, were larger, less basophilic than those of the active animals. The cytoplasm lost its basophile character and if granules were present in the cytoplasm they did not stain with acid or basic dyes. In the hibernating frogs the nuclei were more or less triangular in shape and there was usually only one nucleolus present. In the active frogs the nuclei were round in shape and contained several nucleoli. The nucleus and cytoplasm showed definite affinity for basic dye, while the inclusions of the cytoplasm were very acidophilic.

Kater (1927) compared the skin and stomach of the hibernating frogs with the active animals. The author's findings on the glands of frogs proved to be very similar to his work.

The results of these observations on the glands of frogs, do not justify the conclusion that the function or lack of function of any one of the ductless glands is a direct cause of the tendency to hibernate. Undoubtedly the ductless glands have an important part to play in the development, duration and awakening from hibernation, but it is the writer's view that the functioning is displayed only as a regular phase of the seasonal cycle.

### *Chromatophores*

A change of color is directly associated with dormancy. The color of the hibernating frog was a dull black. The frogs used as controls lived until January, and were black also. When the experiment began in October, these frogs were a light tan color, but changed to black as the season advanced. These changes of color are induced by an alteration in the form of the chromatophores. The migration of pigmented cytoplasm within the melanophores produced the darkening of the color of the skin. Green skin darkens because the melanophores expand, and when the limit of expansion is reached the skin of that region is very dark.

Change of color is also influenced by various external and internal factors. Low temperature, humidity and darkness will cause the expansion of the melanophores and consequently a darkening of the skin. High temperature, desiccation and light will induce a contraction which results in a lighter color. Frogs used for the controls showed a complete change of color during October, November and December, and all the animals unable to make the change in color died. In September, all frogs exhibited a golden brown color.

In these studies, a change of color was not directly due to the action of environmental factors. All experimental animals were kept in the dark, yet all of them passed through the change of color, i.e. dark for winter, changing through shades of brown to green in the spring; a greenish brown in summer and silvery brown in the month of September. All frogs which were unable to make this change of color died.

### *The Muscles*

Although the response of the muscles to electrical stimulation during hibernation, would appear to be a much more obvious matter than the chemical dynamics which these muscles exhibit, the latter seem to have received considerably more attention. Increased tonus of certain groups of muscles cause the hibernant to assume a characteristic position. Muscles have been classed as, the red, those which maintain posture, and the white, those by which movement of the body is accomplished. Rodents roll themselves in their nests, the striped ground squirrel sits on the posterior surface of the hind legs, with the back strongly arched. The various species differ slightly in minor details of position, but in general, the outside curvature of the body almost describes a circle, and only a small gap in the middle preventing the completion of a perfect sphere (Johnson, 1931). This position is maintained by certain muscle fibers. The red muscle fiber which differs chemically and physiologically from the white, is a tonic one and its contraction holds the body in position.

In addition to the seasonal change in the tone of the postural muscle Wacholder (1932) found a seasonal variation in the reaction to acetycholine in the tone of these muscles. This drug causes a strong contracture. In the hibernating mammal, most of the muscles give a stronger contracture for a longer period of time,

with a weaker solution of acetycholine, than at other seasons of the year.

By chemical, histological and pharmacological experiments, Krüger, Duspiva and Fürlinger (1933) have demonstrated these muscle fibers, and have given to them the terms "tonus and tetanus portions." These two types of muscles differ structurally. In winter months, a decrease in the stimulation sensitivity of the tetanus portion was noted. These portions were characterized always by a higher creatin phosphate content. Ferdmann and Feinschmidt (1932) showed that in the hibernating frog there is a decrease in several substances which are important for the contraction of muscles. They believed that decreased excitability of the gastrocnemius muscle of the frog is accompanied by the slowing of decomposition of phosphagen. Phosphagen is not present in the tonus muscle portions (Krüger, 1933). Katz (1934) found a seasonal variation in permeability of frog-muscles to creatin.

The increase in tonus, found in the muscles of *Rana pipiens*, has been mentioned and fully described in the discussion of temperature, earlier in this paper. These positions, characteristic of the hibernating frogs, display the definite reactions of the "tonus portions" during dormancy. According to Krüger (1933), these muscles were pure "tonus fibers" (*rectus abdominis*, *pectoralis major*, and the muscles of the shoulder girdle), and the mixed muscles (*gastrocnemius*, *ileofibularis*, *semimembranosus*) are strongly contracted and if gently touched at the surface of the body, they slowly increase in tonus with a wave-like movement.

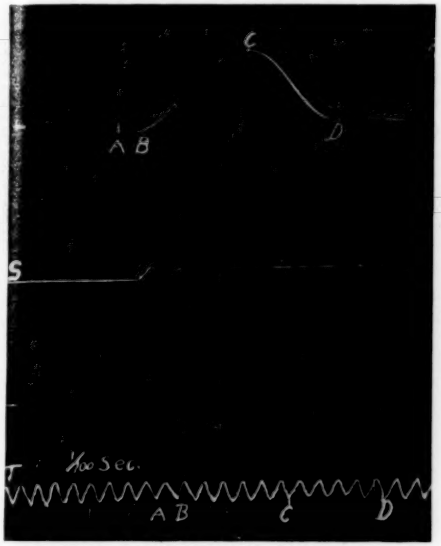



FIGURE 3. A SINGLE TWITCH OF THE GASTROCNEMIUS MUSCLE OF A NORMAL FROG MADE IN OCTOBER

A—B, latent period.
B—C, period of contraction of muscle.
C—D, period of relaxation of muscle.
S, signal magnet indicating application of stimulus.
T, vibration of tuning fork at 100 per sec.

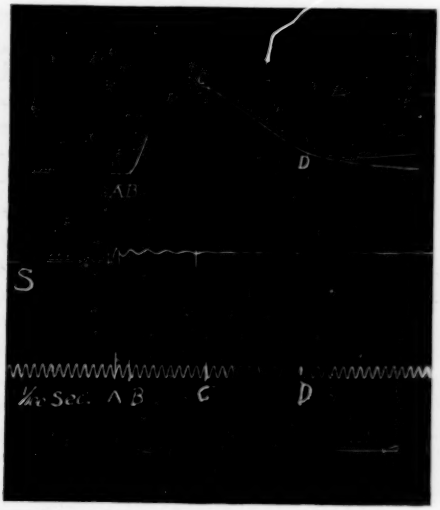



FIGURE 4. A SINGLE TWITCH OF GASTROCNEMIUS MUSCLE OF A HIBERNATING FROG, MADE IN FEBRUARY. CONTRACTION IS RESPONSE TO A "MAKE" OF CURRENT

B—C, shows the apparent secondary release of energy during contraction of muscle.
C—D, shows the delayed relaxation of the hibernating frog.

Cold could not have been the chief stimulus for this increase in tonus in these hibernating frogs, for all the frogs were subjected to the same environment; yet three different results were obtained, i.e. extreme tonic contraction in the hibernating frog of winter, moderate tonus in the sub-active and very little tonus for the active individuals of summer. With the animals used for controls, and kept under atmospheric temperature environment, a progressive increase in tonus was observed as the winter approached, and only the frogs which were influenced by this increase lived through the winter.

A seasonal rhythm was indicated by the curves in response to repeated electrical stimulation applied to the gastrocnemius muscle of *Rana pipiens*. Progressively each month, from January to April, the muscles were able to do more work than was recorded for the preceding month. The averages for the different groups for successive months, showed that the right gastrocnemius muscle of *Rana pipiens*, performed 3870 Gr. mm. of work in January; 5364 Gr. mm. in February, 7721.4 in March, and 13511 Gr. mm. of work in April. With the technique used, the frogs of January fatigued in eight minutes, while the active frogs of April performed work for ten minutes before the onset of fatigue.

The characteristics of the curve for the fatigue of muscles in the hibernating frogs are (1) a contracture, (2) an increased irritability to the tetanizing current when applied to an apparently fatigued muscle, and (3) a decrease in the sensitivity of the muscles to electrical stimulation. To bring a threshold response in the hibernating frog, it is necessary to set the inductorium at point nine, while five points below this number could be effective in finding a contraction in the active animal.

The single contraction of the gastrocnemius muscle of the hibernating frog differs from that of the active individual. There was an apparent secondary release of energy during the contraction, while the curve of the active frog sloped uniformly to the contraction maximum. This departure from the mormal type was constant for the fresh as well as the fatigue muscle. To determine whether or not, this unusual contraction was confined to dormancy, the curves of several other species were made. *Acris*, a non-hibernating frog, two different species of *Pseudacris*, and a narrow mouthed toad (*Gastrophryne*), of the same size range, were used. *Acris* and the hibernating *Rana pipiens*, made totally different curves from those of the other three species. They both displayed the same irregular contraction which was not observed in either *Pseudacris clarki* or *Gastrophryne*. This secondary phase of contraction with *Acris* is more distinct and especially noticeable in the response to the tetanizing current.

### *The Heart*

No agreement was found in the literature, relative to the rate of heart-beat in its relation to temperature. Some observers have found that the curve showing the relationship, is a straight line within the range of normal temperature fluctuations (Taylor, 1931). Others have maintained that the curve is of logarithmic type. Barcroft and Izquerdo (1932) carried out an experiment to determine the effect of changes in temperature on the intact and excised heart of frogs at different seasons of the year. One set of their tests made in December and January, gave a different result from those made in July. The effect of alterations in temperature on the heart-beat of the intact frog in winter, gave results which were incon-

sistent with one another, and in most instances were quite irregular. Usually, for the excised heart of frogs in winter to give a linear curve, it was necessary to use the relationship between the logarithm of frequency of the sinus beat and the reciprocal of absolute temperature between 5 and 20°C. The excised and also the intact heart in summer gave a straight line when pulse rate was plotted directly against temperature.

Taylor (1931) confirmed the above results for his experiment on the intact and excised heart, which he made in June. He has shown also, that the British frogs differ from the South African forms in the response to temperature, and accounts for it in part, by the fact that he found the heart of the South Africa frog subject to vagal tone at temperatures neither very high nor very low, and in the British frog the vagus tone appeared to be absent.

In *Rana pipiens*, used in the writer's experiments, the rate of heart-beat showed a seasonal cycle. The heart-beat of the dormant animal was irregular, and instances of heart-block were common. The ventricular beat failed many times, and a complete missing of the beats was not uncommon. A response to a rise in temperature was often shown in the slowing rather than an increase in the rate of the heart-beat. The same results were observed in the reactions of the sluggish frogs of winter. The frequency of the heart-beat in both the intact and excised heart of frogs of summer, bore a linear relationship to temperature, but in the hibernating frog the tone of the vagus was practically absent. The vagus did not maintain an inhibitory action on the heart-beat at any temperature during the months of winter, though the heart could be completely inhibited for a minute or more in the summer when the tetanizing current was applied to the vagus trunk. Thus it appears that a seasonal rhythm is to be found in the activity of the cardiac muscle of *Rana pipiens*.

### *The Blood*

The relation of the blood to hibernation has been studied but little. Vierodt was the first to describe a change in the number of erythrocytes during hibernation, (Heilkundi, 1854). He found in a marmot at the beginning of hibernation, 5,800,000 erythrocytes per cubic millimeter of blood, but at the end of lethargy only 2,300,000 corpuscles were shown; and he also noted a diminution in the size of the cells.

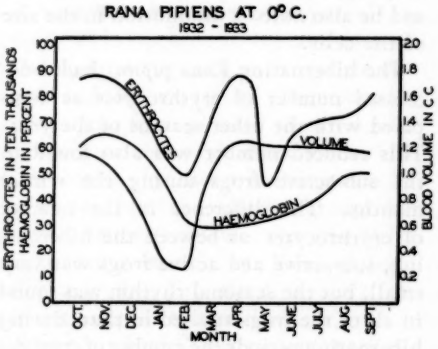
The hibernating *Rana pipiens* had a decreased number of erythrocytes as compared with the other seasons of the year. This reduced number was also found in the sub-active frogs during the winter months. The difference in the number of erythrocytes as beween the hibernating, sub-active and active frogs was very small, but the seasonal rhythm was found in all of the frogs studied in that, during hibernating periods the number of erythrocytes was reduced, while the highest count was made at or near the breeding seasons.

As the period of lethargy approached, a progressive fragmentation of erythrocytes was shown in the blood of the hibernating frogs. When the spring time came there was renewed activity of the hemapoietic tissues and also many small round cells with large nuclei.

Jordan and Speidel (1925) gave the life of any one erythrocyte of a frog as not over one hundred days. Since fragmentation was quite generally observed during the winter, it is possible and reasonable to expect the number of erythrocytes in the dormant animal to be reduced. This reduction in the number of erythrocytes is much greater than it would appear

from taking one blood count, as reference to Graph 1 will show that the decrease in blood-volume parallels the reduction of the red blood cells. The blood-volume also increases in the spring as to the erythrocytes, thereby making a greater increase than is apparent, and thus accounting for the change in the appearance of the erythrocytes in the frogs of spring as compared with those of the late frogs of summer.

Asakawa (1926) found that the erythrocytes of *Bufo* during hibernation showed a higher content of oxygen than




GRAPH I. DIFFERENCE IN THE NUMBER OF ERYTHROCYTES, THE CONTENT OF HAEMOGLOBIN AND THE VOLUME OF BLOOD IN RANA PIPIENS WHICH WERE PLACED IN THE REFRIGERATOR AS RECORDED AT THE DIFFERENT SEASONS OF THE YEAR

at any other time of the year. In my study of *Rana pipiens*, a determination of the content of oxygen in the erythrocytes was not made, but a seasonal cycle was found in the content of haemoglobin, with the lowest percentage present in the hibernating frogs of the winter season.

In addition to the red corpuscles contained in the blood, there are many specialized bodies, called spindle cells. These are supposed to be normally present in the blood of frogs to the number of 16,000 per cubic mm (Nobel (1931)). In frogs of summer these spindle cells are very unstable and are not seen unless great precaution is taken, but in the blood of hibernating individuals they were easily seen. It is thought that they assist in the clotting of the blood; in the dormant frogs this process is easily watched for in some instances the time of clotting covers a period of five minutes, while in the active animal of summer, the process takes place very quickly.

Dubois (1898) and Johnson (1931) thought that in mammals, a very slow circulation was maintained during hibernation. In *Rana pipiens*, the circulation was slow, and during lethargy the blood appeared to be confined to the large vessels and the viscera. By microscopical examination of the foot and tongue, and by dissecting the hibernating frog, a very slow circulation of the blood was shown. Dissection may be performed on frogs in the winter almost without loss of blood. This is of course due in part, to a decreased volume, and to an increase in the specific gravity of the blood of a hibernating animal. The averages shown in the specific gravity of the blood vary from one and five-hundredths in the active animal, to one and seven-hundredths in the hibernating frogs, although different frogs of both groups showed a wide variation.

Since the volume of blood is not constant, any attempt to define it as a percentage of the weight of the body must be misleading. The smaller frogs according to their length, have a volume relatively greater than that of the heavier individuals of the same group. No marked difference was found in the volume of blood between sexes, nor between the experimental and control animals. In the entire series, the frogs used in this study, ranged from fifty-five to eighty millimeters in length, and from nineteen to twenty-five grams in weight at the

beginning of the work. The volume of blood in the dormant frog did not exceed one-fourth cubic centimeter.

SEASONAL CHANGE OF WEIGHT

It is generally believed that there is a considerable loss of weight in the body of mammals during hibernation. There is but a slight agreement in the amount of loss. Many believe that animals which enter hibernation are fat, and when they awaken they have become lean in body (Murray, 1896). Bailey (1926), and Johnson (1931) report slight losses, while a slight, temporary increase in weight has been reported by Horveth (1878) and by Dubois (1896). This condition has been accounted for by Valentin (1896) as caused by an accumulation of moisture, or by fixation of oxygen with retention of carbon dioxide (Pembrey and White, 1896).

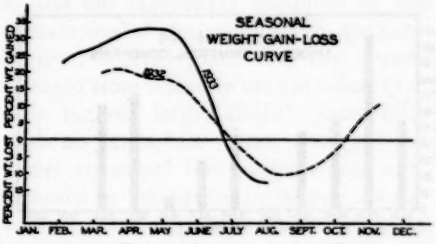
According to Barthelemy (1926), when frogs (*Rana fusca*) were placed in a temperature of from 2–3°C., in October, they gained weight. Above 8–10°C., they remained active and lost in weight of body. He further states that during sexual maturation there is an increase in weight, and that hibernation is ended by sexual maturation.

The frogs (*Rana pipiens*) studied by the writer, when placed under experimental conditions of 0°C. temperature and saturated humidity, without food, gained weight from October to April, and lost weight from April to October, as did also the individuals placed in the atmospheric temperature. They showed an initial gain of weight when placed in the cold in October, and there was no loss from February to March, nor any gains from May to August.

Graph 2 is a curve of the change of weight as related to the seasons, showing April and May to be the months of greatest gains and August and September as that of greatest loss. Had there been an experiment beginning in November and December, this curve might have been completed for the annual cycle.

Although sexual maturation did take place during hibernation, and the coelome of the female was well filled with eggs at the time of emergence, the presence of eggs could not account for the increase in weight, because the increase in weight in the males almost equalled that of the females.

A variation was found in the degree of gain for the different years. The influence of atmospheric temperature upon the frogs before the experiment began, exerted some effect. The dormant frog of 1933, made a greater gain than did those of 1932, and the animals placed in the cold in March 1933, showed the greatest gain with the most sudden reactions.



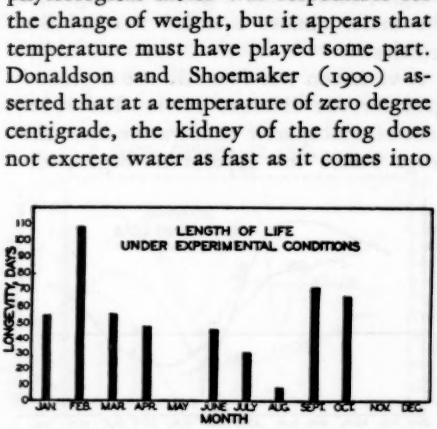

GRAPH II. THE PERCENTAGE OF GAIN OR LOSS OF THE ORIGINAL WEIGHT OF THE FROGS AS RECORDED AT DEATH

(The weight of the animal when placed in the refrigerator was considered as original weight.)

Reference to the weather conditions to which these frogs had been subjected just before the experiments began, shows that in December, the month preceding one experiment, the temperature averaged below normal for the year 1932, while February, the month preceding another group, was the coldest month for 1933, with March the wettest month. These two groups recorded increased gain of weight, with the second group showing

the greatest gains and the most sudden reactions.

The weather reports for February 1932, show unusually mild temperature in all sections of the United States, making the sixth consecutive month with abnormally high temperature for Wisconsin, where the larger number of frogs used in this work were obtained. Undoubtedly some physiological factor was responsible for the change of weight, but it appears that temperature must have played some part. Donaldson and Shoemaker (1900) asserted that at a temperature of zero degree centigrade, the kidney of the frog does not excrete water as fast as it comes into




GRAPH III. THE LENGTH OF LIFE OF RANA PIPIENS UNDER EXPERIMENTAL CONDITIONS

The points on abscissa indicate the months the animals were placed in the refrigerator. Ordinates indicate the average number of days of life.

the body, and therefore the body swells; they also add that this is essentially what occurs during hibernation and that an increase in the content of water is evident in all the tissues of the body during dormancy.

Temperature could not have been the only cause of the gain of weight in *Rana pipiens*. The temperature in the refrigerator was practically constant yet they gave two responses in change of weight. The atmospheric temperature for the controls, for the most part registered in the seventies for the period preceding observations made in June, the initial response of which, was a loss of weight, while the temperature in September registered in the eighty and ninety range and the frogs gained weight. In October the increase in weight made by the controls showed when plotted, an irregular curve, superimposed upon a straight line of gain in weight, until February. Undoubtedly, cold is the optimum condition for the operation of the physiological factors responsible for the gain of weight.

Two hundred frogs grouped in eight experiments, formed the basis of curves, which show a gain of weight for the winter and a loss for the summer, in response to zero temperature and saturated humidity. Since the animals took no food at any time, the increase of weight must be accounted for in the exchange of water in the frogs. "In the normal frog the skin is the one organ of intake, and the kidney the important organ of output" (Adolph, 1933).

Reid (1890) studied the osmotic action of the skin of the frog and reached the conclusion that the easiest osmotic transference of liquid through the skin of the frog is in the direction from the outer toward the inner surface, suggesting the possibility of a definite absorptive force in the living skin. Maxwell (1913) questioned this view, while Townsend (1921) tried to apply the same principle to the behaviour of the skin of the toad and concluded that some sort of absorptive force of the skin seems to be the important factor in preserving the normal content of water in the body of the toad.

Having therefore, removed the influences of food, temperature and humidity by these investigations, and finding still a seasonal variation well defined, we are forced to conclude that a seasonal change of weight must be due to the action of one, or of a combination of several physi-

ological regulators, which we may suppose are to be found in the nervous system and the hormones.

The reactions of *Rana pipiens* and *Rana catesbeiana*, were apparently identical in the seasonal change of weight but the reactions of *Acris*, a non-hibernating frog, were not comparable.

A definite seasonal cycle was clearly shown in this study, in the length of life under refrigeration, and it corresponds with the seasonal gain or loss of weight (Graph 3). The frogs put in the cold in January lived fifty days or more, while one animal lived for one hundred and eight days. The records of March showed decline from the level of figures for January, and the frogs of April lived for forty-eight days. No frogs were placed in the refrigerator in May. The duration of life in the animals of June and July decreased until August, when it was only nine days. September showed a gain of more than seventy days, although the records for October indicate a decline, due perhaps to inability to control experimental conditions in the refrigerator. It should be noted here that although under refrigeration, not all of the frogs hibernated, for some of them remained sluggish, and the term "sub-active" used in this work, is applied to these individuals, in order to include their reactions also.

In view of the findings of these investigations covering the three years of observation on the gains and losses of weight for *Rana pipiens*, it seems reasonable to conclude that a seasonal rhythm is shown in the change of weight in frogs, when placed in constant conditions of temperature and humdity.

GENERAL SUMMARY

A seasonal cycle was observed as follows: (1) Beginning in October and continuing throughout dormancy, the "winter-frogs" gained weight, (2) acquired a darker color of the skin, (3) the tonus of the muscles increased, (4) the volume of blood decreased, (5) the number of erythrocytes was reduced and the percentage of hemoglobin decreased, (7) the muscles soon fatigued, (8) they showed delayed relaxation and an uneven phase of contraction, (9) the heart, at the period of greatest lethargy was irregular and did not respond to changes in temperature according to van't Hoff's law, and (10) it was not completely inhibited by the stimulation of the vagus nerve, (11) the thymus, adrenal gland and the gonads changed from white to orange color, (12) cells became large, slightly basophilic with no acidophilic areas, and (13) the nuclei contained few nucleoli and were flattened or triangular in shape. In the summer, the opposite conditions were observed, beginning about March and continuing through the season. The cells of the glands studied were strongly basophilic, with areas of acidophilic inclusions, and contained many nucleoli. Regardless of environment, the same seasonal rhythm was observed for all *Rana* studied, and true dormancy could be obtained only in winter.

Frogs of non-hibernating forms (*Acris*), did not respond to a change of temperature, in like manner to *Rana*, or undergo a seasonal change of weight.

Hibernation appears to be one phase of the seasonal cycle which is an intrinsic function of the animal itself, and in *Rana pipiens* and *Rana catesbeiana*, is not fundamentally dependent upon environment.

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# IS THERE A "HORMONE OF MENSTRUATION"?

By CARL G. HARTMAN and W. M. FIROR

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UTERINE bleeding, with endometrial necrosis, follows a number of experimental procedures in monkeys, usually after a latent period of 5 to 7 days, namely: (1) after removal of the corpus luteum; (2) after ovariectomy performed at any day of the menstrual cycle, except the first three; (3) after spinal transection; (4) after injection of estrin; (5) after injection of progestin. The cause of menstruation has been stated to be: (1) withdrawal of estrin (Allen, 1, 20, Corner, 2), or (2) withdrawal of progestin (Smith and Engle, 3), or (3) a hypothetical extra-ovarian factor (Hartman, Firor and Geiling, 4). The latter authors questioned the adequacy of the "hormone-withdrawal" theory. In the first place it seemed difficult to imagine a vasomotor mechanism operating days after complete disappearance of the alleged causative factor, estrin, as must of necessity be the case when the latent period comprises 8 or 10 days. Bleeding will also take place in the presence of large amounts of estrin, sudden reduction of dosage being sufficient to precipitate it (Hisaw, 5). Furthermore, induced bleeding, in castrates, often lasts 20 or more days; and with continuous injection of estrin (Hisaw, 5; Engle and Smith, 6; Hartman, unpublished) or progestin (Hisaw, 5, Engle and Smith, 6) over a period of months the bleeding sets in from time to time despite the injections. Likewise, estrin will not always inhibit the normal bleeding if injections are begun after the middle of the cycle (Hartman, 7) but is effective in this respect if begun early in the cycle (Corner, 8; Tietze, 9; Markee, unpublished). These and other facts seemed to argue for an extra-ovarian factor in menstruation.

Finding that the hypophysectomized monkey bled insignificantly or not at all after estrin injections, Hartman, Firor and Geiling in 1930 attributed a bleeding factor to the anterior pituitary. In 1934 and 1935 we took up the matter again for further testing.

Seven females were operated on in October 1934 and in January 1935. Six bled after estrin injections and in all of these more or less of the gland was left in the sella. The estrin (Amniotin Squibb) was generously furnished by the biological supply house of E. R. Squibb and Sons, through Drs. Anderson and Morrell, for these and many other experiments performed in the Carnegie monkey colony over a series of years. Monkey No. 310 did not bleed; at autopsy the anterior pituitary was found intact but the posterior lobe was completely missing. Our previous conclusion of 1930 was, therefore, in error, and the experiment pointed to the vaso-constrictor hormone, pituitrin, as the possible active principle. This seemed a logical conclusion for other reasons also (Hartman and Firor, 10).

In February 1935, seven females were operated on, an attempt being made to remove the anterior lobe completely, leaving the posterior lobe intact. All animals bled after a course of treatment with amniotin. At autopsy the sellae of

four animals were found to contain gland substance of all the lobes; the other three sellae contained posterior lobe and pars tuberalis only. The anterior lobe was, therefore, definitely ruled out as a source of the postulated extra-ovarian factor (Hartman and Firor, 10).

Among a group of animals operated on in January, 1936 there were two that bled spontaneously after the operation, as after castration. At the time of autopsy of animal No. 386, killed at the time of induced uterine bleeding 41 days after the operation, the sella contained small portions of both pars anterior and pars nervosa in a very degenerated condition, to all appearances too necrotic to function.

More nearly complete hypophysectomy marked the experiments of Smith, Tyndale and Engle (11) who removed the entire pituitary with the exception of the stalk and the pars tuberalis. All the animals showing a reddened sex skin bled following the operation and in all of them bleeding followed the cessation of estrin or of estrin plus progestin injections, if nor at the first trial, at least upon the recovery of the subjects with proper nursing care. These careful experiments of the Columbia group, therefore, definitely eliminated the anterior and the posterior pituitary from consideration as causative agents in menstrual bleeding, except in so far as the anterior pituitary regulates the ovarian cycle. There remains only the remote possibility that the stalk may be involved, for according to Fisher, Ingram and Ranson (12) the stalk and the pars tuberalis suffice to prevent diabetes insipidus, and Atwell and Marinus (13) found pressor substance in the stalk of the bovine hypophysis. However, with varying doses of pituitrin, continuously administered for 5 hours, Hartman and Geiling (14) were unable to produce menstrual bleeding in the female monkey. Markee, in this laboratory, likewise failed to induce bleeding by 6 hours continuous injection of pituitrin into the blood stream, although considerable if not complete blanching of the intraocularly transplanted endometrium could be observed.

With the pituitary gland thus disposed of (for the present) as a probable causative factor in menstruation we must look elsewhere for an extra-ovarian "cause," for the "withdrawal of estrin" theory is, in our view, far from complete. Estrin may, indeed, be withdrawn gradually without bleeding resulting, as Markee, working in the Carnegie Colony, has shown during the past year (unpublished). Nevertheless, it may be said that withdrawal of estrin, with consequent rapid regression (shrinkage) of the endometrium, seems to be an essential precursor of the bleeding and constitutes the common denominator of cyclic or induced menstruation. And yet it is a fact that the endometrium of the rabbit or the guinea-pig is stimulated to growth by estrin, regresses upon its withdrawal, yet does not blanch, slough and bleed—that is, it does not menstruate. Wherein lies this difference in response between the endometrium of the lower mammals and that of the catarrhine monkeys and man?

Further investigation of the problem has been greatly facilitated by the researches of Markee (15) on the behavior of the blood-vessels in intra-ocular transplants, observations that find corroboration in the histological studies of Bartelmez (16) on human uteri and of Darron (17) on the blood vessels of the monkey uterus. Markee has noted that the constant and essential event leading to menstruation is not the hyperemia of the endometrium, as we are currently led to suppose, but quite the opposite, namely profound vaso-constriction of the spiral

arterioles, causing death-like blanching of the tissues for a period of 4 to 24, most often 6 to 12, hours preceding the bleeding. The circulation in the basalis goes merrily on, however, during the blanching period. Hence the anemia, and not the preceding hyperemia, constitutes the immediate and indispensable precursor of menstruation. This feature is exactly the same in ovulatory and anovulatory cycles, in animals recently ovariectomized, and in those having been stimulated by estrin. It makes not the slightest difference whether a corpus luteum is present or not—the sequence of events is the same without it as with it. The corpus luteum must be relegated to a minor rôle so far as menstruation is concerned, even though progestin, probably by desensitizing the arterioles of the endometrium, may experimentally prevent the onset of the uterine bleeding (Engle, Smith and Shelesnyak (18)).

A further fact of the utmost importance was brought out by the histological studies of Bartelmez (16) and amplified by the direct observations of Markee (15), namely that but few arterioles in an entire uterus bleed at any given moment. If the mucosa should "menstruate" at all points at once there would be one momentary gush and all would be over in less than two minutes. Neighboring fields supplied by separate arterioles may blanch and bleed at extensive time intervals, hours in fact. This phenomenon might argue against a general hormonal control, though a local hormonal control must be reckoned with. Anyone who has read the classic book on the capillaries by Krogh or considered the action of the "H" substance demonstrated by Thomas Lewis in the control of blood vessel calibre must gain a profound respect for the potency of infinitesimally minute quantities of chemical substances in their vasomotor action. It is quite possible that the hematomata that precede the endometrial blanching release the vasoconstrictors that result in anemia, anoxemia, necrosis, and menstruation. This would make the phase of congestion that produces the hematomata through vascular stasis one of importance in the initiation of the menstrual bleeding.

In summary, our present conviction is that the menstruation problem resolves itself into a study of the spiral arteries of the zona functionalis—morphologically, physiologically, pharmacologically.

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# NEW BIOLOGICAL BOOKS

*The aim of this department is to give the reader brief indications of the character, the content, and the value of new books in the various fields of Biology. In addition there will frequently appear one longer critical review of a book of special significance. Authors and publishers of biological books should bear in mind that* THE QUARTERLY REVIEW OF BIOLOGY *can notice in this department only such books as come to the office of the editor. The absence of a book, therefore, from the following and subsequent lists only means that we have not received it. All material for notice in this department should be addressed to Dr. Raymond Pearl, Editor of* THE QUARTERLY REVIEW OF BIOLOGY, *1901 East Madison Street, Baltimore, Maryland, U. S. A.*

# BRIEF NOTICES

## EVOLUTION

EVOLUTION AND HEREDITY. *Theories and Problems.*

*By Charles E. Walker. A. and C. Black, London; Macmillan Co., New York.* $2.10. 8 x 5½; ix + 222; 1936.

The author of this work is a neo-Darwinian and his book is quite frankly a piece of propaganda (in the correct sense of that much abused term) on behalf of the theory of evolution by natural selection. That the author has exercised great care in his choice of material is evidenced by his bibliography which contains 184 dated items, of which 156 are over twenty years old.

When the doctrine of natural selection was given to the world more than three quarters of a century ago it was believed by those who accepted it to be adequate to account for the entire process of organic evolution but in these modern days of mutations, allelomorphs, and lethal genes it may be seen to be only one (albeit an important one) of many factors interacting to produce an intricate and complicated pattern of organic progress. The author has devoted about one third of his book to a lucid and unprejudiced critique of some of the non-Darwinian theories, among them those of Mendel, Johansen, De Vries, Kammerer, and Harrison (though his treatment of the last is a bit sketchy) and these chapters constitute the most valuable feature of the book. In fact, a layman entirely ignorant of the work of these men could pick up more by reading about them in Dr. Walker's book than by going to the originals.

Walker believes that the laws of Mendelian inheritance hold for chromosomatic but not for cytoplasmic inheritance. Further, saltatory variations such as those on which the mutation theory of De Vries is based are determined by the chromosomes while fluctuating variations such as Darwin hypothecated are governed by the cytoplasm. And characteristics which discriminate varietal forms within the species are chromosomatic, while those which discriminate one species from another are cytoplasmic.

There is an index of five finely printed pages, but the lack of adequate illustrations is a serious defect.

MAN: A SPECIAL CREATION.

*By Douglas Dewar. Thynne and Co., London.* 3s. 6d. 7⅛ x 5; 123; 1936.

In this little book Dewar has tried to show that man, and in fact every living thing, has come into existence by special creation and that every organism is today, with minor variations, exactly as it was when it was created. This theory of course opposes the theory of organic evolution and even more, the theory of the descent of man. The author tries to prove his point by arguments based not on the evidences of special creation, but on the weaknesses and fallacies in the theory of organic evolution. The author's knowledge concerning natural selection and his interpretation of modern theories in genetics has led him to believe that much of Darwinism is "special pleading based not on the rock of scientific proof, but on

the sands of the imagination." He is quite alert in pointing out the "inconveniences and inconsistences" in the Darwinian school of thought and his discussion is worthy of serious consideration.

Dewar is no fundamentalist quack. On the contrary he is a distinguished naturalist of long and varied field experience. While we hold no brief for him or his conclusions, nevertheless we are of the opinion that his observations and ideas cannot be neglected. His strongest conviction is that "it must be realized that since the theories of Organic Evolution and the Descent of Man lack definite scientific proof, they should be taught not as proven facts, but rather as very broad generalizations."

MEMOIR ON FOSSILS OF THE LATE PRE-CAMBRIAN (*Newer Proterozoic*). *From the Adelaide Series, South Australia.*

*By T. W. Edgeworth David and R. J. Tillyard. Angus and Robertson, Sydney.* 7s. 6d. $8\frac{3}{8}$ x $5\frac{3}{8}$; xi + 122 + 13 plates; 1936.

The significance of this work is far greater than one would suspect from its title, for if the conclusions reached by its authors are upheld they will go far toward revolutionizing our ideas concerning the ancestry of the Arthropoda.

The Upper Proterozoic strata which constitute the Adelaide series lie in a horizontal position, substantially the same as when first deposited. There has been no tilting, no faulting, and no metamorphosis, and consequently its fossils are in a remarkable state of preservation. Until their discovery in 1928 the only pre-Cambrian arthropod fossil was *Beltina danai* described by Walcott in 1899 from Montana, but these specimens were so fragmentary that it is not possible to fix their taxonomic position with any certainty. On the other hand, the new genus *Protadelaidea* here described has been so well preserved that its construction has presented no very great difficulty.

Briefly the conclusions arrived at by the authors are as follows: The Arthropoda were already differentiated as a phylum in the Lower Proterozoic from which no fossils have as yet been recovered. But this primitive arthropod did not resemble the annelids so much as some of its modern descendants do, for it had but few segments and these were mostly cephalic. The thorax and abdomen were but slightly developed and bore no appendages. From this primordial ancestor there have been four distinct lines of descent—one to the crustacea, one to the trilobites, one to the pycnogonids, and one to the Arthrocephala, a new class of which *Protadelaidea* is the type. From the Arthrocephala all the other types of arthropod have been developed, but they are not directly descended from *Protadelaidea*.

The book is profusely illustrated and contains an exhaustive bibliography, but there is no index.



THE MAMMALIAN FAUNA OF THE WHITE RIVER OLIGOCENE. *Part I. Insectivora and Carnivora. Transactions of the American Philosophical Society held at Philadelphia for Promoting Useful Knowledge. New Series, Vol. 28, Part I.*

*By William B. Scott and Glenn L. Jepsen. American Philosophical Society, Philadelphia. Distributed by University of Pennsylvania Press, Philadelphia.* $2.50. $11\frac{3}{4}$ x 9; 153 + 22 plates; 1936 (paper).

The White River Formation is composed of beds which are located principally in Nebraska and South Dakota. These strata were laid down by rivers in the Oligocene epoch and are unusually fossiliferous. A part of them is known as the "Bad Lands."

The authors have studied the fauna of this region using the specimens in various museums. Except for tortoises, by far the larger part of the fossils were of mammals which were present in great numbers and diversity. The authors feel that too many species have been named and consequently concentrate on generic distinctions. Among the Insectivora the Leptictidae is the most common family and *Ictops* the most abundant genus. The Carnivora include the creodonts, which while now extinct, are as well represented among these fossils as the Fissipedia. Among the latter the Mustelidae are rare and show little diversification, while the Canidae and Felidae are

more numerous. The text is illustrated by excellent plates.

A Study of the Fossil Horse Remains from the Upper Pliocene of Idaho. *Proceedings of the United States National Museum, Vol. 83, No. 2985.*

*By C. Lewis Gazin. Smithsonian Institution, U. S. National Museum, Washington.* Free. 9½ x 6; 40 + 11 plates; 1936 (paper).

A detailed description of the skeleton and teeth of *Plesippus shoshonensis* from a site near Hagerman, Idaho, with measurements of 7 individuals and 3 related forms, viz., skull 18, teeth 17, vertebrae 6, forelimb 30, hindlimb 30. Associated fauna are listed. *Plesippus* apparently bridges a gap between earlier Pliocene horses and Quaternary *Equus*.

## GENETICS

Human Genetics and Its Social Import. *By S. J. Holmes. McGraw-Hill Book Co., New York.* $3.50. 9 x 6; viii + 414; 1936.

The first eight chapters concern general genetics and biometry—perhaps more briefly than might be desired, yet lucidly. The inheritance of certain more important human physical and mental traits is treated in six more chapters. While a mass of excellent material is presented here, certain topics are treated rather briefly (as blindness and deafness), others are omitted (blood groups, taste reactions), and in the highly controversial field of "mental heredity" scant consideration is paid to the rôle of environment.

The final eleven chapters are devoted to various topics of general human biology, such as mate selection, past and present trends in population, national origins, war, cross-breeding, selective mortality, and proposals for practical eugenics. Criticism may be made of his treatment of human "instincts" (pp. 274–277), the hereditary bases of which are extremely doubtful; the theory that replacement of primitive peoples by whites has been due to natural selection (p. 285) which implies superiority of white hereditary traits, when their superiority in cultural traits such as military technique has been so obvious; usage of the word "race" when either culture or nationality is implied.

This book should be a valuable aid in college teaching, with its generous index and bibliography (containing few references to environmentalist works, however) and a series of questions at the end of each chapter.

Die Vererbung Innerer Krankheiten. *By Wilhelm Weitz. Ferdinand Enke, Stuttgart.* RM. 14 (paper); RM. 14.60 (cloth). 9¾ x 6¼; xi + 197; 1936.

This book presents a summary of some of the findings regarding inheritance of disease. From the extensive list of clinical conditions here discussed, it seems that there are only a few known for which someone, sometime has not found an unusual or apparently unusual occurrence in single families. The author is particularly interested in the different incidence of diseases in identical and fraternal twins and here records most of the available data. While he does not actually admit that he believes in the inheritance of predisposition to all the diseases mentioned, yet the book would have greater value if he had selected the actual from the imagined evidence of presumption of heredity. The student will find this a good source of references. There is an index.

Menschliche Erblehre. *Band I. Vierte, neubearbeitete Auflage.*

*By Erwin Baur, Eugen Fischer and Fritz Lenz. J. F. Lehmanns Verlag, München.* M. 15 (paper); M. 17 (bound) in Germany. M. 11.75 (paper); M. 12.75 (bound) outside of Germany. 8⅞ x 6; viii + 796; 1936.

The fourth edition of this classic on human genetics follows closely in form and substance the previous edition (see Q. R. B., Vol. 3, No. 1, 1928). All of the sections have been augmented by material derived from recent discoveries in the field of genetics and the section on Somatic Inheritance by E. Fischer has been completely revised and rewritten. One

addition, not too welcome, is the vague and inconclusive discussion by F. Lenz on racial psychology and racial mental characteristics.

## GENERAL BIOLOGY

GENERAL BIOLOGY.
*By James W. Mavor. Macmillan Co. New York.* $4.00. 8½ x 5½; xxiii + 729; 1936.

A LABORATORY MANUAL IN GENERAL BIOLOGY.
*By James W. Mavor and Leonard B. Clark. Macmillan Co., New York.* $1.75. 11 x 8¼; v + 201; 1936 (paper).

LABORATORY DIRECTIONS IN COLLEGE BIOLOGY.
*By W. H. Wellhouse and G. O. Hendrickson. F. S. Crofts and Co., New York.* 85 cents. 10¾ x 8; 82 + 10 charts; 1936.

In writing *General Biology*, Doctor Mavor has aimed at stating

> . . . simply and clearly the main facts and principles on which a sound and teachable course in biology can be based. Such an aim requires that the balance between the botanical and zoölogical portions be such as to provide for a real grounding in each of these. The order in which the material is presented has been chosen both with regard to the logical development of the subject and to a close correlation between laboratory and class room work.

The book is divided into five parts: (1), Nature of Life; (2), Plant Life; (3), Invertebrates; (4), Vertebrates, (5), Principles. These discussions are entirely conventional in character. The volume has the virtue of clarity, however, and should prove understandable even to the college freshman. Each chapter is followed by a group of "thought-provoking" questions although no specific references to pertinent literature are included outside of the generalized list mentioned in the preface. The illustrations are not particularly distinctive and many of them are copied. There is an index. This book can be recommended, along with many others, as an adequate introduction to the fundamentals of general biology. It is, of necessity, abbreviated and leaves much unsaid.

*A Laboratory Manual in General Biology* has been designed for collateral use with the text reviewed above, with which it is closely correlated. The manual is novel in that it is a combination notebook and instruction syllabus all rolled into one. Certain aspects of the volume are praiseworthy—it covers a wealth of intelligently chosen material, is clearly written and well organized. On the other hand, the reviewer was not favorably impressed with its underlying pedagogy which frequently requires that the student be presented with already prepared drawings and requested merely to label them adequately. This, it would seem, discourages original observation and leaves the laboratory worker with the impression, already far too prevalent, that an organism really looks like a textbook diagram. However, the manual has its points and is to be decidedly advocated if its mother text is adopted.

In contrast to the preceding guide, *Laboratory Directions in College Biology* is composed simply of a series of instructions that cover 33 different topics of general biological interest and make suggestions for their study. The book is less didactic than that by Mavor and Clark. This has an advantage in that it permits the student to follow more readily his own designs and may, God willing, stimulate him to a modicum of original thinking. It has a disadvantage in that it does not leave the user with as detailed an appreciation of the field. A commendable aspect of the Wellhouse book lies in its inclusion of several elementary exercises calling for field work and the observation of organisms in Nature—an aspect of biology usually denied the young undergraduate. One of these studies, entitled "Leadership," is concerned with the flock and herd organization of certain common vertebrates and represents a new and entertaining departure from the staid contents of traditional freshman biology.

GREEN LAURELS. *The Lives and Achievements of the Great Naturalists.*
*By Donald C. Peattie. Simon and Schuster, New York.* $3.75. 9¼ x 6; xxiii + 368 + 20 portraits + 11 illustrations; 1936.

Naturalists are interesting folk. They follow their dictates with a candid fervor

and spirit that sets them apart from the run-of-mine individual who considers it smart, if he considers it at all, to be blasé about his interests. The best way to appreciate the point of view of the Nature scholar is to be one. The next best way is to read this delightful book which captures, in no uncertain terms, the philosophy behind some of the world's great and near-great naturalists. The author says in the Foreword:

> I am writing about the naturalists, distinguished—as well as they can be—from the biologists. These latter I think of as the indoor men, the naturalists as the outdoor men. To put it another way, the naturalists deal with living beings *in situ*—in their active, vital inter-relations; the biologists are more concerned with isolated organisms, living under controlled laboratory conditions . . . I am human enough, moreover, to dwell upon the more piquant personalities. We love, alas, not so much for virtue as for charm.

Throughout its fifteen chapters the contributions and personalities of many men are discussed in an entertaining yet realistic fashion. The author emphasizes his subjects' accomplishments only as they reveal naturalistic tendencies. Such old standbys as Buffon, Réamur, Leeuwenhoek, Linnaeus, Lamarck, Cuvier and Darwin are given, as is their just due, a notable place in the book. In our opinion, however, the volume reaches its greatest heights in its treatment of some of the lesser lights—Bartram, Michaux, Wilson, Audubon, Say, Rafinesque—who roamed through the early American wilderness and did much to establish native biology in this country.

It is patent that this is a book destined for wide reading. Its lack of technicalities along with a general human interest fit it for the intelligent public. On the other hand, biologists will welcome it for its realistic appraisal of men important in the development of their science. An index, selected list of references and numerous illustrations supplement the text.

LIFE OF THE SHORE AND SHALLOW SEA.

*By Douglas P. Wilson. Ivor Nicholson and Watson, London.* 12s. 6d. 9½ x 7¼; 150 + 129 figures; 1935.

This is the best book of its type that this reviewer has ever come across. It constitutes an introductory treatise on elementary oceanography, and while written principally from the standpoint of biology it covers such diverse matters as the tides, the continental shelf, the geological activity of the waves, variation in chemical content of the sea water, etc. Instead of being ordered taxonomically the biological material is arranged ecologically, which makes easier reading for the beginner enabling him to meet the inhabitants of the ocean in their own homes instead of on the shelves of a museum. There is also a very interesting chapter dealing with invertebrate embryology, which is helpful because so many of the ocean's inhabitants have larval planktonic stages totally unlike the adults.

The book has a comprehensive index, and also many illustrations from diverse sources, chief among them being the author's camera. It is an excellent model of what a beginner's book ought to be, whether it be judged from the standpoint of science, literature, or art. Unfortunately for those who live on this side of the Atlantic, only the British shores are treated in this volume. We should invited Mr. Wilson to take up a temporary residence in this country in order that he might produce a companion work dealing with the coastal waters of America.

ENGLAND HAVE MY BONES.

*By T. H. White. Macmillan Co., New York.* $2.50. 8 x 5½; vii + 334; 1936.

This book, the journal of an Englishman, is "a book about the tangible side of country life." For the most part it consists of the author's impressions of and reactions to his experiences with such pastimes as fishing, hunting, and learning to fly. These he has recorded in an unusually vivid style. The entries were made with an eye to publication; therefore, along with the happenings of the day, Mr. White has included bits of legend, some rather sketchy natural history, his experience with some pet snakes, and a varied assortment of other incidents and facts.

The book is in no sense technical and

will prove interesting chiefly to those people who have had similar experiences, and especially to those who have a passion for fishing, or for flying, or, in general, a sportsman's love of the country. It is not a record of a specific period with a beginning and an end. It is, instead, a record of a typical year of the author's life. The primary purpose is not merely to present his experiences, but, rather, to tell about things observed with "seeing eyes" for the benefit of "people who have lost them." All things considered, it is an interesting though not a profound book. No bibliography or index.

EXPERIMENTAL MODIFICATIONS OF THE EFFECT OF ULTRAVIOLET LIGHT ON PROTOPLASM. I. AMOEBA PROTEUS. *University of California Publications in Zoology, Vol. 41, No. 8.*

*By W. A. Black. University of California Press, Berkeley.* 35 cents. $10\frac{1}{4}$ x $6\frac{3}{4}$; 28; 1936 (paper).

In this paper the effect of ultra-violet radiation on amoebas immersed in various salt solutions is reported. The investigation was stimulated by the studies of earlier workers who claimed that a change in the ionic environment of an amoeba so modified the internal relationships that any effects of irradiation were fundamentally altered as a result. To get at this question the author used light of a wavelength equal to or less than 2540Å and irradiated amoebas in various dilute solutions (chlorides of Na, K, Mg, and K). The general conclusions reached were (1) that the protoplasmic changes resulting from this treatment were typical of injury and not characteristic for irradiation; and (2) that the results did not support the earlier contention mentioned above.

METHODS OF TISSUE CULTURE IN VITRO. Containing following article: *Outlines of Histological Methods with Special Reference to Tissue Culture*, by *Clayton G. Loosli*, 55–81.

*By Ralph Buchsbaum. University of Chicago Press, Chicago.* $1.00. 9 x 6; [8] + 81; 1936 (paper).

The first part of this book is designed to present

> . . . a simplified method of tissue culture for a single investigator in an ordinary biological laboratory. All of the requirements for best growth have been included, but the attempt has been made to eliminate procedures of doubtful value, . . . and to recommend equipment which is as inexpensive as possible.

This section is clearly written and should be of great assistance to the biologist who wishes or must learn the tissue culture technique and equip his laboratory without personal assistance. The second part of the book discusses in an elementary yet competent manner the essentials of ordinary histological methods. Taken all in all the volume is a useful addition to technical biology and should find many friends among laboratory workers. The text material is supplemented by a bibliography of selected references to guide the reader into deeper aspects of the subject if desired.

THE MICROSCOPE. *Sixteenth Edition Revised and Enlarged by the Addition of a Chapter on Micro-Incineration.*

*By Simon H. Gage. Comstock Publishing Co., Ithaca.* $4.00. 9 x 6; viii + 617; 1936.

This widely-used and standard textbook (previously noticed in Q. R. B., Vol. 7, No. 3, p. 348) has been revised in its sixteenth edition by adding a section on the method of reflecting ultra-violet radiation by mirrors coated with aluminum, and a chapter on micro-incineration. The latter discussion aims at ". . . giving definite information concerning the presence, amount and location of the fixed mineral salts in the tissues and cells of the body." These additions to the present volume bring the subject matter up to date and augur well for the continued popularity of the book among biologists and medical men.

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung 452. Methoden der Süsswasserbiologie.* Containing following articles: *Die Massenzucht von nannoplanktischen Grünalgen als Futter für Wassertiere,*

by Einar Naumann; *Neue Einrichtungen und Arbeitsmethoden im limnologischen Laboratoriumsbetrieb*, by Einar Naumann; *Technik der Unterwasserbohrung auf Bohrfähren*, by Erich Wasmund; *Quantitative Methoden zur Untersuchung des Nannoplanktons*, by Hans Utermöhl.

*Urban und Schwarzenberg, Berlin.* RM. 13.50. 10 x 7; 213; 1936 (paper).

The scope of the limnological material included in this number of *Abderhalden's Handbook* is sufficiently indicated by the subtitles for the different parts.

## HUMAN BIOLOGY

MIGRATION AND ECONOMIC OPPORTUNITY. *The Report of the Study of Population Redistribution.*

*By Carter Goodrich, Bushrod W. Allin, C. Warren Thornthwaite, Hermann K. Brunck, Frederick G. Tryon, Daniel B. Creamer, Rupert B. Vance, Marion Hayes and others. University of Pennsylvania Press, Philadelphia.* $5.00. 10 x 6¾; xvii + 763 + 11 plates; 1936.

This book presents the results of a study undertaken "to consider what movements of population within the United States might be necessary and desirable and what part, if any, the government should take in encouraging them." The book is divided into two parts. The first examines the conditions of those regions where the standard of living (measured by the number of radios and telephones and the income tax return) is below the average of the country as a whole. These are the southern Appalachian coal plateaus, the old cotton belt, the cutover region of the Great Lakes states and the great plains. In this part are also included chapters on the trends of labour demand in the several branches of occupation. The second part of this volume contains a discussion of migration in this country, the attempts in England, Germany and Russia to promote migration to more-favoured regions and the similar measures enacted in this country during the present administration. The authors have apparently gone to great pains to derive from the meagre data available an adaquate and objective analysis of the conditions.

The findings are not surprising and may be summarized as follows: (1) the above mentioned regions and the prevalently agricultural, have suffered the most from the present crisis and show the greatest natural increase in population; (2) the chances of future increase in employment are greater in urban communities and for industries than they are in the rural region and for occupations such as mining and agriculture; (3) centralizing industries show no intention of decentralizing nor to move to the above named regions. Therefore, the authors argue, since there is so little hope that the people of these regions can obtain the means of relieving their distress, let us move the superfluous individuals to other and more favored regions. Just how this is to take place and what will be the consequences, the authors do not state either clearly or definitely, although they note that in other countries government interference has been practically ineffective, and so far the various New Deal schemes likewise. Without doubt this book is an important contribution to the knowledge of present economic conditions but, as the authors note, much more information is necessary before any definite plan regarding migration can be made. Included in the book are numerous useful tables and maps.

THE MESOLITHIC SETTLEMENT OF NORTHERN EUROPE. *A Study of the Food-Gathering Peoples of Northern Europe during the Early Post-Glacial Period.*

*By J. G. D. Clark. The University Press, Cambridge; Macmillan Company, New York.* $9.00. 10 x 7¼; xvi + 284 + map in pocket; 1936.

Archaeologists and all those interested in pre-history are deeply indebted to the author for this very excellent treatise. One cannot praise too highly the care and thoroughness that has gone into the preparation of the volume, not only in the textual matter but also in the numerous illustrations (photographic plates, drawings, charts, and maps) and in the excellent bibliography. While it has long been recognized that physical environment greatly influenced the development

of primitive cultures, it has been less generally understood that in the area under consideration changes in environment in the last few thousand years have been "so profound as to alter its influence on cultural development and so rapid as to afford a natural time-scale for the dating and synchronizing of human culture." The region, bounded on the west by the mountain backbone of Britain, on the south by the highlands of Southern Germany, on the north by the mountains of Scandinavia, and open to the east by the great plains of Russia and Siberia, presents evidence that during the early post-glacial period there were "three groups of cultures, three traditions, distinguished by differences of origin and by adaptation to differing types of environment." Food-gathering formed the economic basis of all these cultures.

Of the many interesting phases relating to the Mesolithic peoples that are presented in this book we call attention to only one: namely, the microscopic analysis of ancient pollen grains. Such analysis gives very precise information concerning the general drifts of forest development. In a series of seven appendices additional data are presented in condensed form. The volume is well indexed and in a folder inside the back cover is an enlarged map of the Maglemose culture.

HEADS AND TALES.

*By Malvina Hoffman. Charles Scribner's Sons, New York.* $5.00. 9½ x 6¾; xx + 416; 1936.

Malvina Hoffman's tale of the execution and installation of the type portrait statues that constitute the Hall of Man of the Field Museum in Chicago makes a most interesting and diverting book. It is a *melanges* of autobiography, travel, gossip, philosophy, ethnology, anthropology, art, and what else have or want you. Its success in the best seller class is well deserved. We recommend it most heartily.

The book illustrates in a remarkable way that biologically curious attribute of man—the *general* competence of the professional. The competence that the professional in any field, art, science, or literature, acquires *by training*, is necessarily in the nature of the case a highly specialized competence. Yet it has been repeatedly observed that when a really competent professional is suddenly called upon to do a job of work in a field remote from that for which he is specially trained, he turns out a perfect performance. He exhibits, in short, *general* competence. Miss Hoffman was by training (under Rodin among others) a sculptor, highly successful in a distinctly specialized department of that art. Without any implied derogation it is reasonably exact to say that when she took on this Field Museum job she knew but little more about physical anthropology than the proverbial catfish, and certainly no more than the average art student, which isn't much. Nevertheless in the end she finished a first rate piece of *anthropological* work, over and beyond its artistic merits.

HANDBOOK FOR RECREATION LEADERS. *U. S. Department of Labor, Children's Bureau, Publication No. 231.*

*By Ella Gardner. U. S. Government Printing Office, Washington.* 15 cents. 9⅛ x 6; vii + 121; 1936 (paper).

In this well indexed pamphlet we are told how to teach children, and adults as well, to play. First, the discussion is of community parties and picnics and reeks with helpful suggestions about stimulating the shy and the bored into the general spirit of fun. The bulk of the pamphlet embodies descriptions and rules of over two hundred games ranging from riotous out-of-door sports to singing games and tests of mental skill. The last part consists of poems and songs with suggestions for dramatizing them. The gay and abandoned artistry with which our great humanitarian government shows us how to have fun is charmingly demonstrated in the illustration on facing page, which faces page 78 in the original.

MEASURES OF MEN. *Ten Specialized Studies in Physical Anthropology in Mexico, Central America and the West Indies. Middle American Research Publication No. 7.*

*By Harold Cummins, Mary S. Lane, Stella M. Leche, Ruth Millar, Inez D. Steggerda and Morris Steggerda. Department of Middle American Research, Tulane University, New Orleans.* $5.00. $10\frac{3}{4}$ x $7\frac{1}{2}$; 331; 1936 (paper).

Steggerda gives a brief resumé of the work of three investigators, including himself, on a total of 1050 Mayan Indians of Yucatan. He and Millar compare finger and hand dimensions among Mayans, Negroes and whites. Together with Cummins, the finger prints of 127 Mayans are analyzed in detail, showing a lower frequency of whorls than heretofore found among North American aborigines. The Steggerdas and Lane analyse in detail the palm prints of 224 Mayans, comparing them generously with other series. They resemble most closely the Chinese.

Professor Cummins describes fully the controversy over Valsik's scheme of adding the four main line formulas of the palm to make an index—the "papillar number." Americans have considered this methodology invalid. The author suggests adding the values of lines A and D alone. He presents also a retabulation of H. H. Wilder's collection of palm prints, to the terms of the revised methods.

Four papers by Leche present dermatoglyphic data on five series as follows: 78 Aztecas, 78 Mixtecas, 50 Zapotecs, 100 Chamulas, 116 Tarascans. All were selected to minimise white ancestry. All but the last series include two determinations of functional lateral dominance—eyedness and handedness—which Dr. Leche is correlating with asymmetry in palm and finger patterns with promising results. Three series include 22–28 physical measurements, taken after instruction from Dr. E. A. Hooton.

RASSE UND HEIMAT DER INDOGERMANEN. *By Otto Reche. J. F. Lehmanns, Munich.*

8 Marks (bound); 6.50 Marks (paper). $9\frac{3}{4}$ x $6\frac{1}{2}$; 216; 1936.

The author, professor of anthropology and ethnology in the University of Leipzig, presents in this book a careful and critical investigation, carried on for a period of thirty years, of the much discussed problem concerning the race and original home of the Indo-Germanic (Indo-European) peoples. Believing that comparative philology, archaeology and prehistorical research may indeed contribute much to the study of the subject but that the problem can finally and decisively be solved only by the science of anthropology, he investigates first of all

the racial composition of the various Indo-European peoples both in Asia and Europe, reaching the conclusion that they are fundamentally Nordics. A similar examination of the remains of the population of the most important neolithic cultures of Europe shows these likewise to have been preëminently men of the Nordic race. Proceeding then to a search for the home and origin of this Nordic race he finds that the special morphological, physiological and psychological racial characteristics all point to western, northwestern and central Europe, and not to eastern Europe or western Asia (Siberia, v. Eickstedt), as its home—and hence also as the home of the original Indo-Europeans—and to the latest glacial (Würm) period, the late Mousterian or Aurignacian as the time of its origin. The Nordic race is a product of selection (*Züchtungsprodukt*) of the latest European glacial period (ca. 100,000–80,000 B.C.).

[At this point Reginald the Office Boy suggested that if we wanted to save space, as we always do, why didn't we in place of the above paragraph just say *Heil Hitler*, and inform the reader that the book is provided with 113 illustrations, 5 maps, an index of authors and a subject index.]

GROUP SETTLEMENT: ETHNIC COMMUNITIES IN WESTERN CANADA. *Volume VII. Canadian Frontiers of Settlement.*

*By C. A. Dawson. Edited by W. A. Mackintosh and W. L. G. Joerg. The Macmillan Co. of Canada, Toronto.* $4.50. 10 x 6⅝; xx + 395; 1936.

This volume deals with five group settlements (Doukhobors, Mennonites, Mormons, German Catholics and French-Canadians) made in the Canadian west where their numbers comprised from half to all of the population. These ethnic groups, forming "cultural islands" were each bound together by language, sectarianism, nationalism and collectivism in various combinations which they were determined, almost fanatically, to maintain. The author, professor of sociology at McGill University, and his assistants have produced an interesting study of the social organization of these groups, and of the influences which in the past were (and still are) at work either in preserving or breaking down their permanency. Statistically the colonies have been more than ordinarily successful. There are minor exceptions, especially in those regions where soil and climatic conditions have been adverse. Formal institutions developed quickly in these homogeneous groups and the pioneers early acquired a sense of security. On the other hand, in recent years, with the rapid development of western Canada, "bloc communities" have been a source of much friction with the government. According to the author "it seems clear from the evidence analysed that the unplanned play of external forces in the long run tends to eliminate much, perhaps all, of the distinctiveness of separatist colonies." The volume contains numerous illustrations and a large amount of tabular material. It is well indexed.

THE SCIENCE OF SOCIAL DEVELOPMENT. *A Study in Anthropology.*

*By F. A. Brooke. Watts and Co., London.* 12s. 6d. 8½ x 5½; xiv + 337 + 5 plates; 1936.

This book is a summary, in a rather detailed outline form, of the progress of civilization. It is divided into two parts. The first and larger is concerned with the probable origin and development of the customs, manners, and institutions of man which comprise his culture. The second part shows how the generalities set forth in the first part can be applied specifically. The author has chosen for this illustration the countries of the British Isles, with emphasis upon the development of society in England.

Mr. Brooke has not attempted to present original theories. His material has been drawn from many sources which are acknowledged in abundant footnotes. What he has achieved, however, is a bird's-eye view of man's social development. He does not assume that "the last word has been said in this study of human progress"; but he does maintain that "on the whole it is an accurate account of the institutions and systems that came into being and were developed during the long and arduous progress of human society."

The material is well organized and is presented simply and clearly; but the outline form which contributes clarity detracts somewhat from the interest as it imparts a decidedly pedagogical flavor. This is at cross-purposes with the author's original intention since the book was written for the layman in anthropology. The book contains an index and a rather extensive bibliography.

ASPETTI DEMOGRAFICI DEI GRUPPI CONFESSIONALI IN UNGHERIA. *Con Particolare Riguardo agli Ebrei.*

*By Stefano Somogyi. Instituto per l'Europa Orientale, Rome.* 8¾ x 6; 238 + 1 folding chart; 1936 (paper).

The author compares certain demographic characteristics of the Hungarian Jews with those of the remaining Hungarian population. He finds that, as a group, the Jews marry at an older age, exhibit lower fertility, lower infant mortality and higher masculinity than do the Hungarians belonging to other religious denominations. Are these characteristics due to race and religion? The author thinks not, but believes that they are a consequence of the fact that the economic status of the Hungarian Jews is higher than that of the general population. In addition, he points out that historical evidence contradicts the widespread belief in the racial purity of the Jews; in Hungary, at least, they are as much an ethnic mixture as the remaining peoples. Therefore, he concludes, the Jews should not be considered racially different from the rest of the population, but rather as a religious denomination which is an integral and important constituent of the Hungarian nation.

By analogy to other studies on population it can be assumed that the author is essentially correct in his interpretation of the cause of the demographic differences between Jews and non-Jews. However, more precise and detailed evidence is necessary. There is an adequate bibliography.

CONTROL IN HUMAN SOCIETIES.

*By Jerome Dowd. D. Appleton-Century Co., New York.* $3.00. 8¾ x 5¾; xvii + 475; 1936.

This book, primarily to be used as a college text, is divided into four parts. Part I concerning the origin of control, explains that folkways and public opinion are the most effective means of social control when society is in the genetic stage. As society passes later into the telic or conscious stage the leaders realize that organization forms a more efficient control, guided in part by the other two forces. Part II surveys the history of social control which may be roughly divided into three eras: paternal control, the rebellion against paternal control, and the rise of democratic or social control. Part III is an analysis of the problems of social control in play and recreation, education, industry, marriage and family, religion, control in the state, and in the arts. The last section is devoted to a discussion of principles of control applied to the present chaos in the Western world. The reactions of the Western world to the World War and the following depressions are outlined and the author optimistically feels that out of the chaos and many rapid changes that have taken place there will arise some era of peace and stability with a socialized form of government that will be useful and helpful to mankind.

There is an index and a guide to the study of social control (24 pages) which gives reading references for each chapter, and a number of questions. The book should be a valuable reference and textbook.

LIVING HIGH. *Or at Home in the Far Andes.*

*By Alicia O'R. Overbeck. Lovat Dickson and Thompson, London.* 10s. 6d. 8½ x 5½; xii + 307 + 14 illustrations; 1936.

This is a grand story, told by the Baltimore-born wife of an American mining engineer who took a job with a company operating high, high up in Bolivia—so high that everybody's physiology had to be readjusted to a deficiency of oxygen if they were to go on living at all. They stayed there six years—and being the kind of persons they were, found these "good" years. A better account of the powers of

physical, physiological, and psychological adaptiveness inherent in the human organism would be hard to find than is embodied in this delightfully written account. Children were born and grew in this harsh environment and loved it. The book ends with "no complaints to make, no grievances to air."

> But in Bolivia we left behind us that which we can never regain—that shining part of life when nothing is dull or drab, that part of life when you can dance all night and ride all day without weariness, when long muleback trips over cruel hard trails, when biting cold, when breath-taking altitude, when stark isolation are adventures, wonderful as a fairy tale. To this brightness we must all say *adiós* sooner or later; so let us say it cheerfully and in grateful spirit for having been allowed to live it to the full.

WORKERS' NUTRITION AND SOCIAL POLICY. *International Labour Office. Studies and Reports Series B (Social and Economic Conditions) No. 23.*

*International Labour Office. League of Nations, Geneva; World Peace Foundation, 8 West 40th St., New York.* $1.50. 9½ x 6¼; vii + 249; 1936 (paper).

At the nineteenth session of the International Labour Conference in June, 1935, the following propositions were laid down: (1) that adequate nutrition is a basic factor in the health and well-being of the workers and their families; (2) that there is widespread evidence that larger numbers of workers in town and country are not sufficiently or suitably nourished, even in industrially advanced countries; and (3) that a proper consideration of workers' nutrition would help to solve some of the difficult social-economic problems of today. The general scope and character of this report is based upon these propositions. It is international in character and gives many data concerning the consumption of foods in different countries (by income groups), analyses of nutritive properties of workers' diets, production of foods, prices, market organization, family incomes, etc. A section is included on the co-operative movement. Very nearly one-third of the report is taken up with a series of appendices in which additional data are arrayed in tabular form.

THE ISNEG LIFE CYCLE. I. BIRTH, EDUCATION, AND DAILY ROUTINE. *Publications of the Catholic Anthropological Conference, Vol. 3, No. 2.*

*By Morice Vanoverbergh. Catholic Anthropological Conference, Washington.* $1.75. 10 x 7; 81–186; 1936 (paper).

This paper is one of a series that the Catholic missionary has published on the Isneg, a head-hunting tribe inhabiting one of the subprovinces of Northern Luzon, P. I. The whole life of the Isneg, from cradle to grave is dominated by fear of spirits. Father Vanoverbergh, in the course of his daily routine, has had exceptional opportunities for observing the effect which superstitution, magic, and spirit worship have had upon their behavior. Both pagan and Christian Isneg seek some explanation different from the natural one for any event that does not look ordinary to them. The birth of twins in a family is a dilemma. Two children at once must be the product of two men—even if the father knows his wife to be a virtuous woman. The problem, however, is settled at once and definitely by one of the infants being killed. One of the most interesting sections of the paper is that on the moral code of the Isneg when the writer assumes the garb of an Isneg moralist "merely to give a better perspective to the whole treatment of this very important subject."

THE STUDY OF MAN. *An Introduction. Student's Edition.*

*By Ralph Linton. D. Appleton-Century Co., New York.* $3.00. 8¾ x 5¾; x + 503; 1936.

As a professor of anthropology, the author of this volume felt that there was no one text-book "broad enough in its scope to provide beginners with a grounding in the essentials of Anthropology." He has written a book, therefore, to overcome this difficulty. The book is really a survey of the field of anthropology. Man, his origin, his racial differences, his culture, his society, are all considered, but in a broad, general manner. Furthermore, the author has succeeded in showing the relationships be-

tween these various aspects. Realizing that anthropology is still a young science, he has attempted to present facts and theories in an unbiased discussion and to avoid extravagant claims. In fact, it is so unbiased that it tends frequently to raise questions rather than to reach conclusions.

Although this book was designed primarily as a text for beginning students in anthropology, it may also serve the layman as an introduction into the field of sociology. It is not technical and is written in an interesting readable style. Both bibliography and index are included.

YALE UNIVERSITY PUBLICATIONS IN ANTHROPOLOGY. *Numbers One to Seven* and *Eight to Thirteen.*

*Edward Sapir and Leslie Spier, Editors. Yale University Press, New Haven.* Numbers One to Seven $2.00. 9¾ x 7; 143 + 1 plate. Numbers Eight to Thirteen $2.50. 9¾ x 7; 163. 1936 (paper).

Cultural anthropologists may well congratulate themselves upon the commencement of the Yale University Publications in Anthropology, under the able editorship of Edward Sapir and Leslie Spier. In this notice it will be possible only to mention the titles and authors: Population Changes among the Northern Plains Indians, by Clark Wissler; Regional Diversity in the Elaboration of Sorcery in Polynesia, by Peter H. Buck; Cultural Relations of the Gila River and Lower Colorado Tribes, by Leslie Spier; Hopi Hunting and Hunting Ritual, by Ernest Beaglehole; Navaho Warfare, by W. W. Hill; The Economy of a Modern Teton Dakota Community, by H. Scudder Mekeel; The Distribution of the Northern Athapaskan Indians, by Cornelius Osgood; Profane Literature of Buin, Solomon Islands, by Richard C. Thurnwald; An Outline of Seneca Ceremonies at Coldspring Longhouse, by William N. Fenton; The Shawnee Female Deity, by C. F. Voegelin; Human Wolves among the Navaho, by William Morgan; Musical Areas in Aboriginal North America, by Helen H. Roberts; Rank and Potlatch among the Haida, by George Peter Murdock.

ENCHANTING WILDERNESS. *Adventures in Darkest South America.*

*By Hans Tolten. Translated from the German by Ferdi Loesch. Selwyn and Blount, London.* 15s. 8½ x 5½; 285 + 23 illustrations; 1936.

An interesting, if in some respects rather unpleasing account of the struggles of a young German to establish himself as a planter in the northern territories of Argentina some dozen years or so ago, and after failing in that enterprise, of his efforts to eke out a living in any way. It is full of adventure, sometimes a bit gruesome, and gives a fairly clear and definite picture of the country and its indigenous native inhabitants. On the whole perhaps the most striking thing that comes out of it is the extraordinary parallelism between the way in which the whites there, intent upon exploiting the country, are treating the Indians, and the way in which the whites of North America treated the native Indian population here a century or so earlier. The sensitive reader finishes the book with a feeling of nausea and degradation. And smug folk still prate of the "dignity of man." A considerable section of the book deals with egret hunting—again a most unpleasant topic. The thoughtful biologist and student of population will find much in the book to ruminate over.

BEVÖLKERUNGSFRAGEN. *Bericht des Internationalen Kongresses für Bevölkerungswissenschaft Berlin, 26 August—1 September 1935.*

*Edited by Hans Harmsen and Franz Lohse. J. F. Lehmann, Munich.* 22 marks; 25 percent discount outside of Germany. 9¾ x 6¾; xxvi + 972; 1936.

This stout volume containing some 125 papers presented before the somewhat nationalistic International Congress on Population sponsored by the Germans in 1935, is divided into two parts. In addition to the seven opening addresses Part 1 contains papers on the following sub-

jects: the international significance of the declining birth rate; internal migration and urbanization; change relationships between population and administrative development; marriage and the family; theories of population movements; methods of demographic statistics. Part 2 treats: anthropological and racial problems; differential fertility; race hygiene, heredity and eugenics; state demographic problems; and public health problems.

The individual papers are published in the language in which they were presented but are provided with abstracts in two or more other languages (English, French, German, Italian or Spanish). The volume is supplied with a list of the names and addresses of the participants, and author and subject indices.

YEAR-BOOK OF LABOUR STATISTICS 1935–36. *International Labour Office, Geneva.* $1.50 (paper); $2.50 (cloth). (U. S. Agents: World Peace Foundation, 40 Mt. Vernon St., Boston). 9½ x 6¼; viii + 227; 1936.

The contents of this volume were formerly included in the International Labour Office Year-book. As in previous years it presents the official statistics on employment and unemployment, hours of labor, wage scale, cost of living, workers' family budget, emigration and immigration and labor disputes for a number of countries and for the several groups of occupation. In the preface it is noted that since the sources of information, i.e. the statistical bureaus of the individual nations, have great diversity of scope in collecting the data, international comparisons are "extremely difficult and only possible with considerable reservations; fluctuations within the same country are generally comparable, . . ." This is one of the unfortunate aspects of this type of statistics and greatly limits the utility of such publications as this. There is an appendix giving the population of the different countries. The sources of information are listed.

LE DESTIN DES RACES BLANCHES. *Deuxième Édition. By Henri Decugis. Librairie de France, Paris.* 42 francs. 9¾ x 6¼; x + 565; 1936 (paper).

The fact that a second edition of this book has been printed within a year of the first is sufficient indication of the interest that it has aroused. As it was pointed out in the review of the first edition (cf. Q. R. B. Vol. 11, No. 1, 1936), a book on social and economic trends based on facts and presented in a simple and straightforward manner is uncommon and therefore justly deserves whatever popularity it can achieve. In this edition, the statistical data have been brought to date and six new chapters have been included. These discuss the ethnic composition of the Western nations, the monetary crisis, trends in marriage and divorce, increasing subordination of individual rights, and changes in political ideals. The new chapters do not add materially to the quality of the book, but they do serve to emphasize that the author is one of the few remaining liberal thinkers.

TRAIL-BLAZERS OF SCIENCE. *Life Stories of Some Half-Forgotten Pioneers of Modern Research. By Martin Gumpert. Translated from the German by Edwin L. Shuman. Funk and Wagnalls Co., New York.* $2.50. 8 x 5¾; viii + 306; 1936.

This series of biographies has been written to show "the great battle which mankind has waged within the short period of recorded history in a single domain—that of physical science." The author emphasizes the "ineradicable opposition to genius" which persecuted these great men from the sixteenth century almost to the present day.

The list includes Cardan, Vesalius the anatomist, Servetus who discovered the circulation of the blood through the lungs, Swammerdam, Wolff, Lamarck, Mayer, Jackson and Morton, von Pettenkofer and Harvey Cushing. In each sketch the biography of the individual and his achievements is accompanied by a discussion of the social, political, scientific and religious conditions of the day. Our only criticism is of the confusion that results when the author allows himself to digress too far along these lines.

CHINESE COOKERY BOOK.

*By S. K. Cheng. The Shanghai Restaurant, London.* 3s. 9d. $7\frac{1}{4}$ x $4\frac{3}{4}$; viii + 102; 1936.

Either you like Chinese cooking or you don't. There is no half-way position. If you don't like it this review should not even be read since the reviewer is so prejudiced in its favor that he fears he cannot record here an unbiased judgment. If you do like it get this book for it gives many recipes and instructions for the home preparation of dishes such as those served by the Shanghai restaurant in London. All of these recipes make entertaining reading. One, among many others, which struck the fancy was that for *Tun Suin Arp Peen* calling for 6 oz. duck meat, 1 oz. bamboo shoots, 2 oz. onions, 1 oz. water chestnuts, $\frac{1}{2}$ oz. mushrooms, 1 oz. celery, 1 oz. Chinese pickled onions, sour sweet sauce, and $\frac{1}{4}$ oz. Wun Yee (lichen).

The book is to be recommended to interested parties with the sad but realistic reservation that the American housewife who can produce tasty Chinese dishes in her own kitchen is yet to be found.

APPRAISING PHYSICAL STATUS: THE SELECTION OF MEASUREMENTS. *University of Iowa Studies in Child Welfare, Vol. 12, No. 2.*

*By Charles H. McCloy. University of Iowa, Iowa City.* $1.00 (paper); $1.35 (cloth). $9\frac{1}{8}$ x $6\frac{1}{8}$; 126; 1936.

This study is devoted to an analysis and evaluation of measurements used in appraising the physical status of the child. The author believes that in estimating the health of a child, it is just as important to appraise his physical features as it is to study his family history, or the results of a physician's examination and laboratory analysis.

There is a review of the principal measures available for determinations of weight, fat, limb girths and breathing capacity. In recommending procedures, the author keeps in mind the practical demands of the school or clinic situation. Thus the prediction of optimum weight is based upon simple measurements of height, weight, chest circumference, width of hips and width of knees. Standard landmarks, instruments and techniques of measurement are described and illustrated.

TRUANTS. *The Story of some who deserted Medicine yet triumphed. Based on the Linacre Lecture Delivered at Cambridge, 6 May, 1936.*

*By Lord Moynihan. The University Press, Cambridge; Macmillan Co., New York.* $1.40. $7\frac{1}{2}$ x $4\frac{1}{4}$; 109; 1936.

Thomas Linacre, founder and first president of the Royal College of Physicians, was known as a scholar and philosopher as well as physician. It was as a corollory to this fact that Lord Moynihan in a commemorative address quite fittingly spoke of others who were trained in medicine but became "truant disciples to science, to literature, to law, to the State." In this book he accounts briefly for several of these versatile gentlemen, and there are probably but few people who will not find several surprises among these truants. Copernicus, Galileo, Robert Boyle, John Locke, Berzelius, Thomas Young, Linnaeus, Clemenceau, General Leonard Wood, Rabelais, Conan Doyle, Robert Bridges, Oliver Wendell Holmes are some of the better known names to be found.

NORTHEASTERN AND WESTERN YAVAPAI. *University of California Publications in American Archaeology and Ethnology, Volume 34, Number 4.*

*By E. W. Gifford. University of California Press, Berkeley.* $1.50. $10\frac{1}{4}$ x $6\frac{3}{4}$; v + 108 + 1 folding map; 1936 (paper).

The Yavapai Indians prior to reservation days inhabited a large tract of land in central and western Arizona. Because of their wide geographical range they are considered to offer an unusual opportunity for a study of adjustments to various environments by a hunting and gathering people. Information relating to the subsistance, material culture, social organization and customs, religion and various characteristics of this tribe of Indians is given in the form of notes as acquired from a few informants (13 in number) most of whom had lived together with members of other tribes for many years on the San

Carlos Apache reservation. This source of information seems to the reviewer inadequate for an interpretation of the history of Yavapai culture.

WORLD POPULATION. *Past Growth and Present Trends.*

*By A. M. Carr-Saunders. Oxford University Press, New York.* $4.50. 8½ x 5½; xv + 336 + 2 folding charts; 1936.

This work has been born of a desire to evaluate the world's population in 1650 and also at the present day, to classify the populations as to age, race and sex, to account for the changes observed to have taken place between the control dates, and to draw inferences from these conclusions upon which prognostications may be based. It contains a detailed examination of those factors which one would expect to determine changes in population, such as birth and death rates, war and migration, specific fertility and birth control. The logistic curve as a description of observed population growth is not discussed. The book is adequately indexed and lucidly written.

THE BANTU TRIBES OF SOUTH AFRICA. *Reproductions of Photographic Studies. Vol. IV, Section 11, Plates XLI–LXXX. The Vachopi of Portuguese East Africa. With an Introductory Article on the Vachopi; a Bibliography, and Descriptive Notes on the Plates by Henri-Philippe Junod.*

*By A. M. Duggan-Cronin. Deighton, Bell and Co., Cambridge.* 25 shillings. 11¾ x 8¼; 59 + 40 plates; 1936 (paper).

The Vachopi have a distinct and genuine language of their own. This and other cultural traits as well as certain physical characteristics indicate plainly that from an ethnological point of view they must be considered as a distinct Bantu people which may formerly have been much more numerous than at present. Today they number between 150,000 and 200,000 souls. This section of Volume IV on the Bantu tribes, containing a chapter on the language and culture of the Vachopi, a bibliography, and 27 very handsome reproductions of photographic studies of the Vachopi with explanatory notes, was made possible through the generosity of the Carnegie Corporation acting through the Research Grant Board of the Union of South Africa.

AFRICA'S GOD. *II. Dahomey. Anthropological Series of the Boston College Graduate School, Vol. I, No. 2.*

*By Joseph J. Williams. Boston College Press, Chestnut Hill, Mass.* $1.00. 9¾ x 6⅝; 101; 1936 (paper).

The author finds that "while the tribal religion (in Dahomey) was basically monotheistic, the superimposed animism constantly distracted attention from the Supreme Being and tended, in ever increasing degree, to becloud the real relation of the intermediary creature towards the Creator, until eventually a phase of real idolatry seemingly asserted itself and the creature became the active recipient of the cult of 'latria' belonging only to God." The author is rather critical of the work done by Herskovits on Dahomean religious beliefs. There is a short bibliography, and an index.

THE ESKIMOS.

*By Kaj Birket-Smith. Translated from the Danish by W. E. Calvert; translation revised by C. Daryll Forde. Foreward by Diamond Jenness. E. P. Dutton and Co., New York.* $5.00. 8½ x 5½; xiv + 250 + 33 plates; 1936.

The author, having lived among the Eskimos as one of them for nearly two years and visited northern Alaska and Greenland many times, seems to have both a thorough and a sympathetic knowledge of these people. The book should be quite fascinating for the general reader as well as being a valuable ethnological document. A summary of Eskimo tribal groups, rules for pronunciation of Eskimo words, and a long bibliography are to be found in the appendix.

YOUR FOOD SUPPLY.

*By Allen H. Lester and Donald G. Ferguson. American Institute for Economic*

*Research, Cambridge.* $1.00. 8½ x 5½; 62; 1936 (paper).

Believing that during the next few months food prices will rise 15 to 50 per cent above present levels, the authors of this enlightening and helpful booklet devote the first and larger portion to a discussion of the reasons for price increases in each of the principal classes of foodstuffs. The second part is devoted to a discussion of budget protection, for housewives as well as for institutions. Some specific suggestions are made; but for more detailed information the authors list in an appendix a number of government publications "of assistance in buying, preparing, and storing food."

DATING PUEBLO BONITO AND OTHER RUINS OF THE SOUTHWEST. *Pueblo Bonito Series, Number 1.*

*By A. E. Douglass. National Geographic Society, Washington.* $1.00. 10 x 6½; 74; 1935 (paper).

This is an extremely interesting account of the dating of Indian ruins in the Southwestern U. S. From a comparison of rings found in the roof logs and beams, with the rings of other logs or trees of known age, an overlapping of ring sequences has made it possible to construct a calendar which not only dates quite precisely these prehistoric ruins, but also gives direct information concerning climatic conditions of those times. This method of crossdating tree rings is illustrated serially through several plates from the year 698 to 1929 A.D.

WOMAN, WINE AND A SAUCEPAN.

*By Elizabeth Craig. Chapman and Hall, London.* 3s. 6d. net. 7¼ x 4¾; 189; 1936.

People versed in the noble lore of cookery have known for many years that a number of dishes are improved by the addition of certain wines in proper proportions. The present volume should delight the gourmet and amateur chef since it gives many recipes for comestibles which call for wine in their preparation. A glance through the pages shows that sherry, madeira and claret are extensively used and that particular attention has been given to soups, egg dishes, fish, game, poultry, meats and sauces. There is also a section on the serving of wine and a number of suggestions for making those "long and tall" summer drinks. For interested parties this book can be recommended as a useful and readable guide.

RETZER HEIMATBUCH. *Volume 1: Von der Urzeit bis zum ausklingenden Mittelalter* (1526).

*By Rudolf Resch. Stadtgemeinde, Retz.* 12.40 marks. 9¼ x 6½; xvi + 430 + 90 + 1 folding map. 1936.

Retz is situated in Austria near the Czechoslovakian border, slightly northwest of Vienna. This nicely produced and illustrated volume contains the history of the community from prehistoric times, through the founding of the town (*circa* 1050), its destruction during the Hussite wars (1278–1425), its rebuilding and to the end of the Gothic period. An appendix supplies a list of source material, genealogies and a history of the houses standing in 1935 and the successive occupants from the time of their construction.

ESSAYS IN ANTHROPOLOGY *Presented to A. L. Kroeber in Celebration of his Sixtieth Birthday, June 11, 1936.*

*By various authors. University of California Press, Berkeley.* $6.50. 10¼ x 6½; xxiii + 433; 1936.

These thirty-six essays by as many different authors are too varied in both subject matter and quality to be discussed as a group. Some speculations on New World prehistory by A. V. Kidder appealed to us as being the paper of the most general interest of any in this collection. He maintains that civilization sprang up independently in the Old and New Worlds although he is not a polyenist as regards new world culture. A bibliography of Professor Kroeber is included.

88 DANISH DISHES *or Dining in Denmark.*

*By Hetna Dedichen. Andr. Fred. Høst and Son, Copenhagen.* Kr. 3. 7¾ x 5¼; 65; 1936 (paper).

These recipes have been collected as those most typical of Danish cooking. It is evident that soup, fish, pastry, and cake play a more prominent part in the Danish cuisine than in the American. Recipes for preparing vegetables and meat are also given, and the description of the "Cold Table" is both colorful and a bit overwhelming in its suggestion of plenitude, not to say plethora. The Danes, like all Scandinavians, eat hearty.

THE FUTURE OF OUR POPULATION.
*By C. P. Blacker. The Eugenics Society, London.* 6d. 8½ x 5½; 14; 1936 (paper).
A brief analysis of trends in rates of births and deaths in England and Wales since 1710, together with estimates of population through the year 2000 (in detail through 1949) based upon the assumption that these trends will continue. Net reproductive rates of 1933 are given for ten other countries. That of Britain is shown to be insufficient for maintenance of its population. Changes in age composition are explained.



DER URWALD RUFT WIEDER. *Meine zweite Forschungsreise zu den Ituri-Zwergen.*
*By Paul Schebesta. Anton Pustet, Salzburg and Leipzig.* GM. 6. 9 x 6; 208 + 40 plates + 1 folding map; 1936 (paper).
While far from a systematic ethnography, this travelogue through the country of the eastern Ituri pygmies in the Belgian Congo contains many valuable observations of the natives' material and social culture. Unfortunately it is printed in German rather than Roman type. The 89 photographs are excellent and the index generous.

THE RISE OF MAN THROUGH HIS HANDIWORK.
*By David Reisz. Better Education Association, Cleveland, Ohio.* 35 cents. 7⅛ x 4½; 36; 1936.
An effort to popularise the art and handicrafts of paleolithic Europeans.

REPORT ON THE WORKS PROGRAM.
*Works Progress Administration. Division of Research Statistics, and Records. U. S. Government Printing Office, Washington.* 11¼ x 9; v + 106; 1936.
THE CHILD. *Monthly News Summary, Vol. 1, No. 1.*
*U. S. Department of Labor, Children's Bureau, Washington.* 10½ x 8; 28; 1936.
JUVENILE-COURT STATISTICS AND FEDERAL JUVENILE OFFENDERS, 1933. *Based on Information Supplied by 284 Juvenile Courts and by the United States Department of Justice. Seventh Annual Report. Bureau Publication No. 232.*
*U. S. Department of Labor, Children's Bureau. U. S. Government Printing Office, Washington.* 10 cents. 9¼ x 5⅞; iv + 114; 1936 (paper).



## ZOÖLOGY

GEORG WILHELM STELLER. *The Pioneer of Alaskan Natural History.*
*By Leonhard Stejneger. Harvard University Press, Cambridge.* $6.00. 8¼ x 5¾; xxiv + 623 + 30 plates; 1936.
This is a breath taking book. It is the narrative of adventure, the magnificent audacity of which is unsurpassed in scientific history. In 1741, before the intellectuals of Europe and America had ever heard of Alaska, Georg Wilhelm Steller, then 32 years of age, was discovering that country, exploring it, and collecting scientific material from it for the Academy of Sciences at St. Petersburg, and for his friends Linnaeus and Gmelin. Except for the exploration of Lower California by the Jesuit Eusebio Francisco Kino half a century earlier, Steller was the first naturalist to explore any part of the Pacific coast of North America.

Steller is remembered today chiefly for his discovery of the Alaska sea lion, because it perpetuates his name, but this was the least significant of the four marine mammals that he was the first to see, the others being the northern fur seal, the sea otter, and the Arctic sea cow. Of these, the sea cow became extinct within a generation, the sea otter is practically extinct today, and the fur seal would have disappeared long ago had it not been for

the multilateral treaty in which six nations united for its protection.

Despite its pinniped appearance the sea otter is not a seal at all, but a true otter, related to the weasels, and we can imagine Steller's surprise in finding it living in the ocean and feeding on fish and kelp; but the discovery of the sea cow must have astonished him even more, for the order of Mammalia to which it belongs, the Sirenia, consists of but two other legitimate species and two more of doubtful validity, all of them occurring only in tropical regions, and whose nearest living relatives appear to be the elephants. Berings Island on the edge of the Arctic ocean would be one of the last places where one would expect to find such a creature.

Lack of space here precludes mention of Steller's exploration of Lake Baikal, with its remarkable pseudo-marine fauna in fresh water; of his heroism during that winter in the Arctic when the crew of the expedition of whose health he was in charge was being decimated by scurvy—that severe penalty laid by nature on those who tried to penetrate too deeply into her secrets during the eighteenth and some previous centuries; or of his arrest for an offense which he did not commit; or finally of his death in Kamtschatka as a result of treatment received while under arrest. Let it suffice to say that Dr. Stejneger has rediscovered one of the romantic figures of history whom the world has forgotten, but who now lives again in the pages of this remarkable book. Dr. Stejneger has spent a quarter of a century searching three continents for material, and the result is a masterpiece of biographical literature.

WILD LIFE IN SOUTH AFRICA.

*By H. A. Bryden. George G. Harrap and Co., London.* 15s. 8¾ x 5½; 283; 1936.

Mr. Bryden is a famous old hunter and field naturalist, in the best old-fashioned sense of the word. He has ranged all over South Africa. A keen intelligence and wide knowledge supplements the naïve, sharp-eyed curiosity of the child about everything that lives. The resulting book—in part reprinted from articles published in such journals as *The Field* and *The Spectator*—is a delight to the biologist who is one because of a fondness for life and living rather than for mathematics or physical chemistry. The ecologist particularly will find it a mine of information useful to his purposes.

Space is lacking to demonstrate by quotation the extraordinary interest of the book. But we cannot refrain from noting briefly one observation. In accordance with an established custom a Bushman set fire to the dry grass in a glade, to provide new growth that would attract game.

> As the flames spread they dislodged numberless butterflies and moths, which, mounting into the air, flickered bewildered hither and thither above the red fire and the dun smoke. As if from space had come, attendant upon those teeming delicacies, scores of brilliant rollers, birds of about the size of and having something of the aspect of a jay, resplendent in the most gorgeous plumage of blues, greens, lilacs, violets, and browns, in many exquisite shades. These birds seemed to appear the instant that the fire had begun; their numbers grew rapidly, and they were now feasting greedily on the insects thus provided for them. The rollers, which at this dry season were mostly of one species (*Coracias caudata*), the lilac-breasted roller (the others having gone north with the rains), had been summoned from over a space of some miles of the neighbouring forest by some agency swifter even than the telegraph. How was it managed? How did the tidings spread? Possibly in much the same fashion in which vultures, soaring far apart in the sky, learn from the downward flight of their nearest neighbour that the flesh banquet awaits them below. Anyhow, in three or four minutes from the starting off the Bushman's fire these forest rollers, which are by no means plentiful over a given space, were on the spot and hard at work.
>
> The fire swept on; it drove the Bushman's wife and her babe pell-mell from their *scherm*; and still the devouring birds accompanied the fire, hawking fiercely at every butterfly and insect that they could lay their bills to. In twenty minutes the flames had swept the grass glade clean, the birds had vanished, the show was over.

The book unfortunately lacks an index.

COLOR CHANGES OF ANIMALS IN RELATION TO NERVOUS ACTIVITY.

*By G. H. Parker. University of Pennsylvania Press, Philadelphia.* $1.50. 9 x 6; x + 74; 1936.

This interesting discussion, originally presented as a Joseph Leidy Lecture at the University of Pennsylvania, reviews the recent studies of Professor Parker and his students "on the means of activating

color-cells in the higher animals and on the significance of these processes for the workings of the nervous system." The author is especially interested in suggesting that neurohumors (chemical activators that pass from the nerve terminal and elicit a response by the effector cell) may be important elements in the general activity of the nervous system. These neurohumors have been studied in certain fish where it has been shown that they play a definite rôle in the control of chromatophores. The following sentence, presenting succinctly the author's conception of neurohumoral activity, aids in picturing the relation to the nerve impulse:

> The conception to which we are finally led respecting the control of melanophores in *Fundulus* is as follows: this control is accomplished through two sets of autonomic nerves, concentrating and dispersing, and, though it is what would be termed a strictly nervous control, it is nevertheless based upon a special type of hormone, a neurohumor, which ordinarily passes directly from the nerve terminal to the effector cell, the melanophore, over an almost submicroscopic distance, but under other circumstances may make its way over stretches of a millimeter or two from its region of origin to distant effectors by way of the lipoid constituents of the intervening tissues.

This is a stimulating and provocative book. It can be recommended to biologists interested in the physiology of the nervous system on the ground that it delineates a new approach and pleads for further investigation. There is an index, a list of references and a number of text photographs.

WHAT PHYSIOLOGICAL PROBLEMS ARE OF INTEREST TO THE MARINE BIOLOGIST IN HIS STUDIES OF THE MOST IMPORTANT SPECIES OF FISH? *Rapports et Procès-Verbaux des Réunions, Vol. 101, 1ème Partie.*

*By W. von Buddenbrock. Conseil Permanent International pour l'Exploration de la Mer. Andr. Fred. Høst and Fils, Copenhagen.* Kr. 1. 10¼ x 8¼; 14; 1936 (paper).

THE MEASUREMENT OF SUB-MARINE LIGHT AND ITS RELATIONSHIP TO BIOLOGICAL PHENOMENA. *Rapports et Procès-Verbaux des Réunions, Vol. 101, 2ème Partie.*

*Conseil Permanent International pour l'Exploration de la Mer. Andr. Fred. Høst and Fils, Copenhagen.* Kr. 3. 10¼ x 8¼; 66; 1936 (paper).

The first part of this 101st report of the *Conseil International* contains one article by Professor von Buddenbrock on important problems for the marine biologist. The author outlines the problems of (1) sense physiology, including rheotaxis, geotaxis, phototaxis, and vertical traveling, (2) salt and fresh water habitats, (3) physiology of blood and respiration, and (4) nourishment. There is no discussion of original research, the pamphlet is merely an outline of problems. The second part of this report comprises six articles by different authors on the phenomena and measurement of sub-marine illumination. The articles are as follows: Submarine Illumination in Relation to Animal Life, by E. S. Russell; The Photo-electric Measurement of Submarine Illumination in Offshore Waters, by H. H. Poole; Light Penetration in the Western North Atlantic and its Application to Biological Problems, by George L. Clarke; Spectral Bands of Submarine Solar Radiation in the North Pacific and Adjacent Inshore Waters, by C. L. Utterback; On the Unit for Radiation in Oceanic Research, by Anders Ängstrom; and The Transparency of Sea Water, by Hans Pettersson.

THE LIFE HISTORY OF COTYLOPHORON COTYLOPHORUM; A TREMATODE FROM RUMINANTS. *Illinois Biological Monographs, Vol. 14, No. 4.*

*By Harry J. Bennett. University of Illinois Press, Urbana.* $1.50. 10½ x 7; 119; 1936 (paper).

This monograph records for the first time the appearance of the trematode *Cotylophoron cotylophorum* on the North American continent; describes its life-history as a parasite of ruminants; discusses its morphology, both in adult and immature stages; and presents a historical resumé of previous studies made on the genus. It is shown that the form displays a typical fluke cycle with miracidium, sporocyst and rediae stages confined to a snail (*Fossaria*); with cercaria and metacercaria acting as the infective stages of the vertebrate host; and with adult worms living in the cow's rumen where they

become sexually mature. The time required to complete the entire life-cycle was found to vary between five and eight months. This includes the period during which the worms are migrating from the duodenum, where the metacercaria excyst, to the rumen.

This study gives every indication of being carefully done. Due to its specialized nature it will have little interest for the general zoölogical reader but should, nevertheless, form a constructive addition to helminthological literature. There are a number of drawings depicting the anatomy of the form and a bibliography.

LARVES DE COLÉOPTÈRES AQUATIQUES DE L'EXPÉDITION LIMNOLOGIQUE ALLEMANDE EN INSULINDE.

*By Henri Bertrand. Archiv für Hydrobiologie. Suppl. Bd. XIV "Tropische Binnengewässer, Band VI." Stuttgart, 1935.* 93 + 11 plates.

A general account of aquatic larvae (Coleoptera) collected by the German Scientific Expedition of Dr. A. Thienemann to Sumatra, Java and Bali (1928–1929). The list of material for each family is preceded by a summary concerning bibliography, biology and systematic study (with dichotomous tables). The illustrations include 39 drawings in the text and eleven plates, concerned with the general morphology and specific features. Thirty-three larval types or genera are described in this work, twelve for the first time. The author, a well-known specialist on dytiscid larvae, working now on the biology of dryopids was helped in diagnosis by comparative study of his own collections and stocks from several museums, chiefly the U. S. National Museum and the National Museum in Paris.

VARIATIONS AND DISEASES OF THE TEETH OF ANIMALS.

*By Frank Colyer. John Bale, Sons and Danielsson, London.* £2 20s. net. 9¾x7¼; viii + 750; 1936.

Sir Frank Coyler, consulting dental surgeon of Charing Cross Hospital and the Royal Dental Hospital of London has given with this volume a taxonomic reference work of great and permanent value, especially for students of comparative anatomy and dental pathology. Variations of the teeth in number and shape, and variation in position are described as found in specimens of the following mammalian orders: Primates, Carnivora, Artiodactyla, Perissodactyla, Hyracoidea, Proboscidia, Rodentia, and Marsupialia. Members of the family Hominidae are not included. Families, genera and even species of these orders are found to have a marked difference in degree of variation. The latter half of the book is devoted to chapters on abnormalities of the teeth resulting from injury and disease. The book is illustrated with over a thousand superb photographs.

References are given as footnotes in the text, but are not summarized into a bibliography. There is an index.

MOSQUITOES OF THE ETHIOPIAN REGION. *I.—Larval Bionomics of Mosquitoes and Taxonomy of Culicine Larvae.*

*By G. H. E. Hopkins. British Museum (Natural History), London.* 15s. 10x7¼; viii + 250; 1936.

This monograph makes available for the first time a comprehensive and up to date account of all the known larvae of Culicine mosquitoes in the whole African region south of the Sahara, in Aden and Southwest Arabia, Madagascar and adjacent small islands, and the islands of the Gulf of Guinea. Introductory chapters on ecology, external morphology and technique are followed by taxonomic descriptions of twelve genera. Since the generic term *Mansonia* is now well established, it seems regrettable that the author has adhered to the earlier *Taeniorhynchus* which is so similar to the name *Taeniarhynchus* applied to a group of worms. This is especially true since the latter holds priority, and although obsolete at the present time is again under consideration for a group of tapeworms. The book is well written and the text is amply supplemented with drawings which show clearly those structures necessary for identification. A supplementary monograph on Culicine adults is due to be published in

1937. There is a list of references and an index to generic, specific, and varietal names.

MORPHOLOGY OF THE INSECT ABDOMEN. *Part III. The Male Genitalia (Including Arthropods Other than Insects). Smithsonian Miscellaneous Collections, Volume 95, Number 14. (Publication 3396).*

*By R. E. Snodgrass. Smithsonian Institution, Washington.* 40 cents. 9¾ x 6¼; 96; 1936 (paper).

This monograph constitutes the third part of Snodgrass' important study. Part I dealt with the general structure of the abdomen and its appendages, and Part II with the genital ducts and the ovipositor. The latter earlier received favorable comment in these columns. (Vol. 9; No. 2; p. 239.) Great care and a skillful selection of text-matter has characterized all three endeavors and entomologists are in the author's debt for making such diverse material readily available to them. The current study presents a straightforward, thorough, and critical account of the anatomical and ontogenetic relationships of the male genitalia of both insects and certain other arthropod classes. Among the insects, the Collembola, Protura, Thysanoptera, Ephemeroptera, Dermaptera, Plecoptera and Orthoptera are discussed. The miscellaneous arthropods are represented by the Onychophora, Pycnogonida, Xiphosurida, Arachnida, Crustacea and Myriapoda. Throughout the text the comparative point of view is admirably stressed and the phyletic interrelations of various groups, as interpreted through the male reproductive system, are emphasized. The volume boasts a good bibliography and a number of helpful illustrations.

THE BIOLOGY OF MAYFLIES. *With a Systematic Account of North American Species.*

*By James G. Needham, Jay R. Traver and Yin-Chi Hsu. Comstock Publishing Co., Ithaca.* $7.50. 9½ x 6½; xvi + 759; 1935.

The Ephemerids have long been recognized as one of the most fascinating of insect orders. This is due, in part, to their interesting life-history which involves the structural and functional degeneration of the imagoes and a remarkable specialization of the nymphs for aquatic life. Despite these attractive features, however, the group has been neglected by entomologists. It is, therefore, a pleasure to welcome this lengthy and authoritative volume on the general biology and taxonomy of mayflies. The book is divided into two sections: one dealing with mayflies in general and discussing their anatomy, embryology, life-cycle, phylogeny and adaptations; the other stressing the systematics and ecology of North American forms. The text material is clearly presented and is supplemented by excellent taxonomic keys, numerous illustrations, and an index and bibliography.

An important addition to entomological reference works.

ZOOLOGICA: *Scientific Contributions of the New York Zoological Society, Vol. 21, Part 2, Numbers 3–11.* Containing following articles: *Bermuda Oceanographic Expeditions. Individual Nets and Data, 1932–1935,* by William Beebe; *Plankton of the Bermuda Oceanographic Expeditions. I.* and *II. Notes on Protozoa,* by G. H. Wailes; *Plankton of the Bermuda Oceanographic Expeditions. III. Notes on Polychaeta,* by Edith Berkeley; *Plankton of the Bermuda Oceanographic Expeditions. IV. Notes on Copepoda,* by Charles B. Wilson; *Plankton of the Bermuda Oceanographic Expeditions. V. Notes on Schizopoda,* by W. M. Tattersall; *Plankton of the Bermuda Oceanographic Expeditions. VI. Bathypelagic Nemerteans Taken in the Years 1929, 1930 and 1931,* by Wesley R. Coe; *Tissue Culture and Explantation in Nature: A Review of Certain Experiments and Possibilities,* by C. M. Breder, Jr.; *Preliminary Note on the Nature of the Electrical Discharges of the Electric Eel, Eletrophorus electricus (Linnaeus),* by C. W. Coates and R. T. Cox.

*New York Zoological Society, New York.* $1.00. 10½ x 7; 60; 1936 (paper).

Seven of the nine articles in this number of *Zoologica* concern the Bermuda Oceano-

graphic Expeditions of William Beebe. An intensive faunal survey of a circular area about eight miles in diameter was made by the use of nets towed at different depths. A list of these nets with time and weather conditions is given, and a diagram showing the location of the area studied and the arrangement of the nets.

The planktonic contents of a sample of the nets are discussed in general and individual papers are devoted to protozoa, polychaetes, copepods, schizopods and bathypelagic nemerteans. The last is particularly good, discussing the specimens in great detail, and illustrated with ten plates.

The paper on tissue culture and explantation in nature deals with the hypothesis "that animal and plant cells when dislodged from their original location *in situ* by natural causes may continue living independently as distinct organic units," and with the difficulties of experimental verification of such an hypothesis.

A Review of Fishery Statistics in Relation to Wholesale-Index. *Conseil Permanent International pour l'Exploration de la Mer. Rapports et Procès-Verbaux des Réunions, Vol. 98.*

*By W. Nellemose. Andr. Fred. Høst et Fils, Copenhagen.* Kr. 4.00. 10⅝ x 8¼; 72; 1936 (paper).

In view of the difficulties engendered by currency fluctuations since 1913 in making comparisons of industries or commodities of different countries, this author has made use of the wholesale price index of the countries as a basis for evaluation and comparison of their fisheries. Since the wholesale price index is theoretically homogeneously calculated for practically all countries, it again theoretically offers not only a means of obtaining directly comparable values within a country from year to year, but when these values are converted into a monetary unit of the selected basic year they are supposed to be directly comparable from country to country.

Numerous tables and diagrams illustrate the relationship between quantity of production, value in pre-war shillings, and current values (values for any year converted into shillings at the current exchange rate) for the fisheries of all countries—excepting Spain—adhering to the International Council. Similar diagrams are shown for the comparison of the four most important species: herring, cod, haddock and plaice.

Forest Insects. *A Textbook for the Use of Students in Forest Schools, Colleges, and Universities, and for Forest Workers.*

*By R. W. Doane, E. C. Van Dyke, W. J. Chamberlin and H. E. Burke. McGraw-Hill Book Co., New York.* $4.50. 9 x 6; xii + 463; 1936.

In preparing the present volume the authors have had in mind the needs of forest supervisors, rangers, park superintendents, timbermen and others for a textbook giving practical information on (1) the insects that attack trees, and (2) certain methods for controlling these pests. Excluding several chapters devoted to the latter aspect of forest entomology, the book is essentially an exetended list of brief discussions, arrangd in taxonomic order, about injurious forms. Each entry attempts to describe the species in question; to point out where and how the tree is injured by it, and to suggest corrective measures.

The book will probably serve adequately as a practical guide for foresters. It will not greatly interest biologists or even general entomologists since no effort has been made to organize the material around theoretical principles. A good index, a selection of references following each chapter, and an appendix listing the common coniferous trees of the United States along with their principal insect enemies form useful adjuncts to the text.

The Freshwater Mollusc Helisoma Corpulentum and Its Relatives in Canada. *Canada Department of Mines. National Museum of Canada, Bulletin No. 79, Biological Series No. 21.*

*By F. C. Baker. J. O. Patenaude, Ottawa.* 25 cents. 9¾ x 6½; 37; 1936 (paper).

First identified by Thomas Say in 1924 as a member of the old genus *Planorbis*, *Helisoma corpulentum* and its allied forms have presented perplexing problems to the naturalist. The present paper brings into an orderly array the Helisomas of the *corpulentum-trivolvis* groups, giving the salient diagnostic features of each form, the type localities and present location of the type material, and a detailed list of the material contained in the museums of the institutions from which specimens were loaned. Most of the specimens figured in the plates (63 figures, 5 plates) are from the collection in the Natural History Museum of the University of Illinois. A close study of these forms and their distribution leads the author to the conclusion that "following the last glacial advance there was an acceleration of evolutionary factors resulting in the appearance of the large species and races of Helisomas so abundantly distributed throughout Ontario and other parts of Canada."

SALAR THE SALMON.

*By Henry Williamson. Little, Brown, and Co. Boston.* $2.50. 8 x 5½; viii + 301; 1936.

This is an excellently written book which tells the story of a five year old salmon called Salar, a very old sea trout named Trutta, and Gralaks the grilse, from the time when they come into the estuary from the Atlantic feeding banks in the early Spring until the river cycle ends after the winter spawning. While the period covered is only one year, the entire life cycle of the salmon is described through fish of various ages.

Mr. Williamson's style is charming, the descriptive passages are exquisite, and the illustrations are delightful. It is obvious that the author has studied the subject thoroughly, but one doubts his knowledge of the mental state of fish.

PHYTOPLANKTON AND THE HERRING. *Part II. 1933 and 1934. Ministry of Agriculture and Fisheries. Fishery Investigations, Series II, Vol. 15, Number 1.*

*By R. E. Savage and R. S. Wimpenny. His Majesty's Stationery Office,* London. 4s. 6d. net. 10½ x 7; 88; 1936 (paper).

This technical paper is a continuation of the work done by Savage and Hardy between 1921 and 1923. With the aid of diagrams and tables it covers two years of monthly observations along the Flamborough Line of stations, where there is found a correspondence of young fish, eggs and diatoms. The patches reached a maximum density of 1,377,300 cells per $m^3$ for *Rhizosolenia* and 2,487,900 cells per $m^3$ for *Biddulphia*. Evidence is brought forward that within a diatom patch there are relatively more eggs and juveniles in the zooplankton than in the area outside. It is suggested that diatom patches may form a nursery for the animal community which may orientate itself to the patch according to its physiological condition.

SPIDER WONDERS OF AUSTRALIA.

*By Keith C. McKeown. Angus and Robertson, Sydney.* 6s. 7¼ x 5; ix + 270 + 33 illustrations; 1936.

This is a superb example of the best type of popular scientific writing. The author is Assistant Entomologist of the Australian Museum in Sydney. His book is a mine of information about the biology of spiders, with special references to Australian forms. It is charmingly written, and illustrated with half-tone plates of photographs, some of which are remarkable. There is a good index, but no bibliography.

We recommend the book most warmly. Every biological library, whether private, or high school, college, or university, should have a copy of it.

COMPARATIVE STUDIES OF THE FLUCTUATIONS IN THE STOCKS OF FISH IN THE SEAS OF NORTH AND WEST EUROPE. *Reports of the Proceedings of the Special Biological Meeting Held on May 11, 1936, at Copenhagen. Rapports et Procès-Verbaux des Réunions, Vol. 101, 3ème Partie.*

*Conseil Permanent International pour l'Exploration de la Mer. Andr. Fred. Høst and*

*Fils, Copenhagen.* Kr. 4. 10¼ x 8¼; 90; *1936* (paper).

Following a meeting in 1935 committees were formed in both Europe and America to collect data which would throw light upon the yearly fluctuations in the stocks of certain commercial fish common to the eastern and western Atlantic. This report embodies the results of the European survey relating to cod, haddock, mackeral, salmon, bluefish, halibut, plaice and hake. Available data were found wanting in some cases and insufficient in the others, but the reports are only intended to be preliminary to further investigations.

DISTRIBUTION OF THE ARGENTINE ANT IN THE UNITED STATES AND SUGGESTIONS FOR ITS CONTROL OR ERADICATION. *U. S. Department of Agriculture Circular No. 387.*

*By R. M. Smith. U. S. Government Printing Office, Washington.* 5 cents. 9¼ x 6; 39; 1936 (paper).

*Iridomyrnex humiles*—the Argentine ant——was introduced into the U. S. around 1890 and has now spread through the Gulf states and California, becoming an economic pest of considerable importance. Omniverous in its feeding habits, it causes losses to orchardists, planters, beekeepers and others, and is especially despised as a house infesting insect. This publication presents the results of studies carried out to determine the present distribution and abundance of the insect in the U. S. and describes in detail a method which has been tested and found effective in several Mississippi localities for the eradication or control of infestations.

STRANGE SEA SHELLS AND THEIR STORIES. *How they are made and grow. How they are colored and the patterns produced. Rare shells. Shells that build a raft. Shells that bore in rocks. Giant shells. The shell that sinks ships, etc.*

*By A. Hyatt Verrill. L. C. Page and Co., Boston.* $2.50. 8¾ x 6; xvi + 206 + 6 plates; 1936.

Anyone, young or old, with a natural curiosity about sea shells will have their interest greatly stimulated and soundly guided by this book. It is not just a descriptive list of various shells, for each shell has its own fascinating story delightfully and simply told. Who would not be interested in reading about, "Shells that Carry Daggers," "Shells that throw out Anchors," or "The Shell that Builds a Raft," or any other of Mr. Verrill's twenty intriguing chapters? The book is illustrated by the author in pen and ink drawings and a frontispiece of nineteen shells in full color. The lack of an index is stupid and deplorable.

BIRDS IN THE WILDERNESS. *Adventures of an Ornithologist.*

*By George M. Sutton. Macmillan Company, New York.* $3.50. 8½ x 6; xiv + 200 + 10 illustrations; 1936.

This simple and charmingly written book, illustrated by the author in extraordinarily beautiful water colors and pencil drawings, is more of an autobiography than a list and description of the birds. Entertainingly and artistically Sutton has described his adventures as an ornithologist from the far north to the swamps and jungles of the south. His accounts of the birds themselves are more interesting because they come somewhat incidentally into the text as adjuncts to the story of how he found them, and along with the delightful descriptions of their natural environment.

DIE BLATT-MINEN MITTEL- UND NORD-EUROPAS. *Bestimmungs-Tabellen aller von Insekten-Larven der verschiedenen Ordnungen erzeugten Minen. Lieferung 3.*

*By Martin Hering. Gustav Feller, Neubrandenburg.* Subscription price for 6 numbers: (Germany and Switzerland) 12 marks; (foreign, except Switzerland) 9 marks. 9¼ x 6¼; 225–336 + 2 plates; 1936 (paper).

A continuation of an illustrated list of European leaf burrows, previous numbers of which have been noticed in Volume 11 of the Q. R. B. As in the preceding numbers the arrangement is in alphabeti-

cal order of plant hosts and here carries the list from *Forsythia* to *Myrica*.

NATURE PROTECTION IN THE NETHERLANDS INDIES. *A Translation from Supplement to Contribution No. 10 of the Nederlandsche Commissie voor Internationale Natuurbescherming. Special Publication of the American Committee for International Wild Life Protection No. 8.*

*American Committee for International Wild Life Protection, Cambridge.* 9½ x 6; 73; 1936 (paper).

Following a brief discussion of animal conservation measures in the Dutch East Indies, field notes are presented for most of the larger reptiles and mammals, including the several anthropoids. The observations concern distribution, behavior, superficial appearance, and numbers, being of particular interest to conservationlists.

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung 453. Methoden der Tierhaltung und Tierzüchtung.* Containing following articles: *Die Züchtung und Pflege von Laboratoriummäusen*, by T. Laanes; *Aufzucht und Haltung der gebräuchlichen Laboratoriumstiere: Amphibien und Reptilien*, by Emil Witschi; *Methoden der Zierfischhaltung und -zucht für wissentschaftliche Zwecke*, by Carl Kosswig.

*Urban und Schwarzenberg, Berlin.* RM. 6. 10 x 7; 118; 1936 (paper).

The first paper included in this number of *Abderhalden's Handbook* describes the methods of rat breeding and management in use in Dr. MacDowell's laboratory at the Carnegie Institution, Cold Spring Harbor; the second and third give thoroughgoing advice on the care of various amphibia, reptiles and tropical fish used for experimental purposes.

REPORTS OF THE PROCEEDINGS OF THE SPECIAL PLANKTON MEETING *held on May 27th, 1935, at Copenhagen and of the Special Meeting held at Videnskabernes Selskab's Premises on May 31st, 1935. Conseil Permanent International pour l'Exploration de la Mer. Rapports et Procès-Verbaux des Réunions, Volume 45.* Containing following articles: *A Review of some Aspects of Zooplankton Research*, by F. S. Russell; *Further Investigations upon the Photosynthesis of Phyto-plankton by constant Illumination*, by H. Höglund and S. Landberg; *The continuous Plankton Recorder: a new Method of Survey*, by A. C. Hardy; *Die Ergebnisse der internationalen hydrographischen Beobachtungen im Kattegat im August 1931*, by B. Schulz.

*Conseil Permanent International pour l'Exploration de la Mer. Andr. Fred. Høst et Fils, Copenhagen.* Kr. 3.00. 10⅝ x 8¼; 61; 1936 (paper).

A REVIEW OF THE PHYLLODOCIDAE (ANNELIDA POLYCHAETA) OF THE COAST OF CALIFORNIA, WITH DESCRIPTIONS OF NINE NEW SPECIES. *University of California Publications in Zoology, Vol. 41, No. 10.*

*By Olga Hartman. University of California Press, Berkeley.* 25 cents. 10¼ x 6¾; 17; 1936 (paper).

LAWS AND REGULATIONS IN SUMMARY CONCERNING SALMON AND TROUT FISHERIES. *Conseil Permanent International pour l'Exploration de la Mer. Rapports et Procès-Verbaux des Réunions, Volume 46.*

*Compiled by T. E. Pryce-Tannatt. Andr. Fred. Høst et Fils, Copenhagen.* Kr. 2.00. 10⅝ x 8¼; 37; 1936 (paper).

GENERAL CATALOGUE OF THE HEMIPTERA. *Fascicle IV. Fulgoroidea: Part 2. Cixiidae.*

*By Z. P. Metcalf. Smith College, Northampton, Mass.* $2.00. 9 x 6; 269; 1936 (paper).

## BOTANY

THE LIVING GARDEN *or The How and Why of Garden Life.*

*By E. J. Salisbury. The Macmillan Co., New York.* $3.00. 8½ x 5½; xi + 338; 1936.

The author of this book (awarded a Veitch Memorial medal) has attempted to reconcile two botanical points of view generally regarded as incompatible with each other: the purely scientific and the purely practical. He presents facts familiar to the botanist and facts familiar to the horticulturist, then points out their mutual interdependence. As he says, he

has "tried to present the plant as a living organism in relation to its garden home, interpreted in the light of the most recent research."

It would be impossible to survey the fields of horticulture and scientific botany in their entirety. Besides, the book is not meant to serve as a source of facts, but rather to provide the primary stimulus to that comprehension which, ever increasing, will further coöperation between the horticulturist and the professional botanist. Only relatively few environmental conditions and their effects are considered, first in relation to the living plant in general, to alpines and rock plants in particular, and with reference to their bearing on some horticultural practises such as vegetative propagation and hybridization.

Since the book was written for anyone interested in plants, technical terms have been almost entirely excluded. This results in some lack of precision; but, says the author, "To the layman the technical term may be just unintelligible jargon. . . Therefore, . . . I hope that . . . the presentation will be accepted as an honest endeavor to avoid both technicalities and misconceptions."

The material is presented in an interesting, readable style. The text is illustrated with fair drawings and excellent photographs. There is an index but no bibliography.

THE REDWOODS OF COAST AND SIERRA.

*By James C. Shirley. University of California Press, Berkeley.* $1.25. 9¼ x 6; 74; 1936.

In the midst of the Coast Redwoods of Humboldt Park there is a highway store where one may purchase gasoline, soda pop, and picture post cards. Conspicuous among the latter are two, one of which bears the caption "The World's Tallest Tree, 368 feet high." The other reads "Redwood Tree over 400 feet high." To the interested and intelligent tourist who inquires "How come?" the clerk replies, "We don't know anything about the trees. We get our post cards from traveling salesmen."

To meet the exigencies of this sort the present work has been prepared by the ranger naturalist of the Yosemite, who is also professor of botany at Phillips University, Enid, Oklahoma. Within its narrow compass there is arranged in an orderly and compact manner all facts which the visitor to the redwood forests wants and cannot get. Not only are the two recent species of redwood discussed from the standpoint of botany, history, literature, etc. but the fossil redwood forests of Calistoga, the Yellowstone, and Laurence Island are included, as well as some other trees, famous for their height and girth, including the eucalyptus of Australia, the kauri pine of New Zealand, the Cryptomeria of Japan, the Chapultepec cypress of Oaxaca, and the banyan tree of India. One looks in vain, however, for any mention of the chestnut tree at Catania under which one hundred horses of Napoleon's cavalry are said to have found shelter from a thunderstorm.

The book is attractively gotten up in a cover made to resemble redwood slab, and is illustrated with photographs from many sources. Not the least of its merits is the demolition of the all too prevalent idea for which no less a person than John Muir seems to have been responsible, that the Sequoia of the Sierras is not a redwood.

BOTANY. *A Textbook for Colleges. First Edition.*

*By J. Ben Hill, Lee O. Overholts and Henry W. Popp. McGraw-Hill Book Co., New York.* $4.00. 9 x 5¾; xiii + 672; 1936.

The present book, an outgrowth of the general botany course at Pennsylvania State College, has been designed to meet the needs of first year college students for a text that covers in considerable detail the fundamental aspects of plant biology. The volume is divided into two parts each corresponding essentially to a semester's study. Part I, entitled "The structure and physiology of seed plants," emphasizes the elements of plant anatomy, metabolism and reproduction. Part II, discusses the classification, distinctive features and ecology of algae, fungi, liverworts, mosses and spermatophytes. Due

to this dichotomy of "principles" from "types" the book should prove very adaptable for collegiate teaching, and, supplemented by laboratory exercises, should provide a thorough survey of the plant kingdom. In the main, the authors succeed in writing clearly yet maturely about their subject. Furthermore, they are not afraid to introduce the student to technicalities. The book exhibits few of the symptoms of spoon feeding so characteristic of many introductory texts. It is well illustrated and indexed.

GROWTH HORMONES IN PLANTS. *Authorized English Translation of Die Wuchsstofftheorie und ihre Bedeutung für die Analyse des Wachstums und der Wachstumsbewegungen der Pflanzen.*

*By P. Boysen Jensen. Translated and Revised by George S. Avery, Jr., and Paul R. Burkholder with the Collaboration of Harriet B. Creighton and Beatrice A. Scheer. McGraw-Hill Book Co., New York.* $3.50. 9 x 6; xiv + 268; 1936.

The English translation of Professor Boysen Jensen's *Die Wuchsstofftheorie* is the first comprehensive review of the literature dealing with the rôle of growth hormones in normal growth and tropisms of plants. As a result of intensive study of the *Avena* coleoptile, methods have been developed to show the significance of the growth hormone in the normal growth of plants and in the different tropisms. Something is known of the properties and methods of preparation of the hormone, but the chemical formula is still empirical. Attempts have been made to explain the mechanism of transportation of growth hormone in plants, but definite quantitative proof is still lacking. Professor Jensen's review of the subject makes clear numerous weaknesses and gaps in our knowledge and thus helps to point the way for future research.

The bibliography is quite extensive and includes the 1935 literature.

DISINFECTION AND STERILIZATION.

*By Ernest C. McCulloch. Lea and Febiger, Philadelphia.* $5.50. 9¼ x 5¾; 525; 1936.

The comprehensive scope of this book may be indicated by a review of the chapter headings: the development of our knowledge of disinfection and sterilization; natural agencies which control microbial populations; germicidal properties of the body, its fluids, and secretions; the destruction of microörganisms by radiant energy emanations; the effect of temperature upon microbial life; pasteurization; other physical agents; how disinfectants are compared; the dynamics of disinfection; the acids; the alkalies; the heavy metals and their salts; the dyes; the phenols, cresols, alcohols and related compounds; the halogens; water purification; sewage treatment; miscellaneous disinfectants; the selection of a disinfectant. Each chapter is followed by a list of references.

While presenting the most up to date information available on the above subjects, it has been the aim of the author to make "no attempt to limit the presentation of data to irrefutable facts and indisputable hypotheses." It has been his hope thereby to stimulate investigation of the many slightly understood aspects of germicidal action. The book has an index.

PFLANZENGEMEINSCHAFT UND UMWELT. *Ergebnisse und Probleme der botanischen Standortforschung.*

*By Paul Filzer. Ferdinand Enke, Stuttgart,* RM. 5. 9 x 5¾; vii + 98; 1936 (paper).

Discusses various ecological matters that the author has observed in his travels. The first chapter considers the struggle for water which plants undergo in a dry region. This is followed by an account of plant life in a German beech forest. The third chapter deals with the struggle for existence that plants undergo in the mountains, and the special protections they have against the cold. Chapters on plants living on the edge of the sea, and on marine algae complete the volume. The book is written for the layman who has a knowledge of the rudiments of botany and wishes to inform himself more thoroughly for his travels.

GROWTH AND SURVIVAL OF DECIDUOUS TREES IN SHELTER-BELT EXPERIMENTS AT MANDAN, N. DAK., 1915–34. *United States Department of Agriculture Technical Bulletin No. 496.*

*By Ernest J. George. U. S. Government Printing Office, Washington, D. C.* 5 cents. 9⅛ x 6¾; 48; 1936 (paper).

The object of this extensive tree planting experiment was to find what kind of trees and what arrangements of trees would furnish the best protection to buildings and orchards on the northern Great Plains from damaging winds and drifting snow. Only the broad-leafed trees are considered in this report, although coniferous species are included in other experiments in the shelter-belt investigations. For the several experimental belts data were obtained on height growth, survival, adaptability and congeniality of species, the most suitable trees for outside rows and the optimum width of shelter belts. Several tables present the actual data of the experiment. There is a bibliography of 14 titles.

FOOD PLANTS OF THE NORTH AMERICAN INDIANS. *U. S. Department of Agriculture Miscellaneous Publication* No. 237.

*By Elias Yanovsky. U. S. Government Printing Office, Washington.* 10 cents. 9⅛ x 6; 84; 1936 (paper).

This publication is a compilation of all the plants known to have been used as sources of food by the Indians of North America, with brief notes as to the ecology of the plant, the part used as food and method of preparation. There is a bibliography and an index which includes both common and scientific names. There is also a table which presents a summary of families, with number of genera and species of each.

URDEUTSCHLAND. *Deutschlands Naturschutzgebiete in Wort und Bild. Lieferung 20.*

*By Walther Schoenichen. J. Neumann, Neudamm.* 2 marks. 10¾ x 8¼; 169–192 + 8 plates; 1936 (paper).

Continuing this beautiful publication noticed in earlier numbers of the Q. R. B., the present section discusses the old trees that still exist in Germany. There are several full page colored prints, reproductions of old masterpieces, and a great many fine photographs of some of the old oaks. About twenty pages are devoted to description, the rest to photographs. There are eight full page plates.

A DICTIONARY OF BRITISH WAYSIDE TREES. *By A. W. Holbrook. Country Life, London.* 7s. 6d. 7¼ x 4½; 236 + 46 plates; 1936.

Arranged in alphabetical order, over sixty species are described in this book. The author attempts to show the layman how to distinguish between the familiar trees of Britain in winter as well as in their summer foliage. The book is illustrated by excellent photographs of the leafless winter trees, and pen and ink drawings of their buds, leaves, and fruit. The author is his own illustrator. There is an index of the trees described, and a glossary, but nothing in the way of bibliography.

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung* 455. *Ernährung und Stoffwechsel der Pflanzen.* Containing the following article: *Methoden der experimentellen Untersuchung mykotropher Pflanzen*, by Elias Melin.

*Urban und Schwarzenberg, Berlin.* RM. 5. 10 x 7; 94; 1936 (paper).

The methods outlined and discussed in this *Lieferung* of the *Abderhalden Handbook* are chiefly those which have been used by investigators of the physiological properties of the mycorrhiza plants. A seven-page bibliography is provided.

BLACK ROCK FOREST PAPERS, VOL. I. *Trends in the Nitrogen, Phosphorus, Potassium and Calcium Content of the Leaves of Some Forest Trees During the Growing Season, by H. L. Mitchell.*

*Black Rock Forest, Cornwall-on-the-Hudson, New York.* 11 x 8½; 29–44; 1936 (paper).

## MORPHOLOGY

THE COMPARATIVE ANATOMY OF THE NERVOUS SYSTEM OF VERTEBRATES, INCLUDING MAN. *In Two Volumes.*

*By C. U. Ariëns Kappers, G. Carl Huber and Elizabeth C. Crosby. Macmillan Company, New York.* $16.00. 10½ x 7⅜; Volume 1, xvii + 864; Volume 2, xi + 865–1845; 1936.

These are volumes to which the adjective "distinguished" may be liberally yet honestly applied for they are notable in their authorship, scholarship, format and organization. Indeed, it is rare that a reference work in any field attains the standard found in the present books. The entire enterprise

. . . is an outgrowth of *Die vergleichende Anatomie des Nervensystems*, which was written by C. U. Ariëns Kappers in 1920–1921. When, in 1926, the present text was begun, the original plan on the part of both Dutch and American collaborators was to translate the German text into English and to make such revision as should be deemed necessary in the light of later literature. However, so great additions had been made to the knowledge of the comparative anatomy of the nervous system during the years following its publication that it soon became evident that, in order to do justice to the situation, much of the text must be entirely rewritten, and additions made to the figures. The idea, then, of presenting a translation was abandoned. Thus . . . the present book offers a new presentation of the material available in comparative neurology, based on the available literature and on results of the research programs of the Institute of Brain Research at Amsterdam and the Laboratory of Comparative Neurology at the University of Michigan, both of which have collaborated in the preparation of the present text.

The first volume, containing an introduction and seven chapters, discusses The evolution and morphology of nervous elements; the comparative anatomy of the spinal cord; the medulla oblongata; the lateral line and acoustic systems; the effectory system of the midbrain and the medulla oblongata; the coördinating apparatus; and the cerebellum. The second volume, in addition to a subject and an author index, has chapters on the mesencephalon and the diencephalon; the telencephalon; and the development of the cortex in mammals. The material of each chapter is presented in taxonomic order with the cyclostomes, plagiostomes, ganoids, teleosts, amphibians, reptiles, birds and mammals typically discussed. An extended and superbly complete bibliography follows each chapter, and citations to the literature are cross-referenced in the text. Many excellent illustrations supplement the discussions.

This book, covering intensively and authoritatively an important field of biological research, will unquestionably find a permanent niche among the classics of scientific literature. It is, in truth, a monumental study.

ANATOMY OF THE HUMAN BODY. *Twenty-third Edition.*

*By Henry Gray (Thoroughly Revised and Re-edited by Warren H. Lewis). Lea and Febiger, Philadelphia.* $10.00. 10¼ x 6½; 1381; 1936.

COMPARATIVE ANATOMY.

*By Herbert V. Neal and Herbert W. Rand. P. Blakiston's Son and Co., Philadelphia.* $4.75. 9 x 6; xxi + 739; 1936.

It would be both pedantic and somewhat ludicrous to review in any detail even a current copy of Gray's Anatomy. This lusty veteran, having withstood, in 23 editions, the rigorous manhandling of 78 years of intensive service, needs neither appraisal nor introduction. It, as the politicians say, "stands on its record." The current edition has been altered by correcting and modernizing the general text; by rewriting parts of the sections dealing with embryology, the central nervous system, the endocrines, and physiological anatomy; by adding new and more effective illustrations, and by appending recent references to anatomical literature. The book is strongly bound and well indexed and will, of course, continue to be the middle-man between cadaver and medical student.

In contrast to Gray's Anatomy which is of ancient origin, *Comparative Anatomy* by Neal and Rand is a new book just beginning its career. Whether the career will be as long and remarkable as Gray's remains to be seen. It is likely, however, that the book is destined for considerable longevity in its own right since it is a usable and skillfully prepared text covering the conventional subject matter of vertebrate morphology. The facts presented have been selected because they

"throw light upon the important problem of man's place in nature and . . . help the student to understand the major functions of his body." A chapter is devoted to each anatomical system (viz., integumentary, skeletal, muscular, digestive, respiratory, vascular, urogenital, endocrinal and nervous) and there are, in addition, brief discussions of the animal kingdom, histology, reproduction and vertebrate phylogeny. The authors rightly emphsize each structural element in terms of its specific function and their treatment, coupled with laboratory dissection, should provide the student with a sound appreciation of the vertebrate pattern. The illustrations are good and the glossary and index helpful.

BONES. *An Essay on the Development and Structure of the Vertebrate Skeleton.*

*By P. D. F. Murray. The University Press, Cambridge; The Macmillan Company, New York.* $2.50. 7½ x 5⅜; x + 190 + 8 plates; 1936.

An extremely interesting, well-written, and critically sound digest of recent work on the morphogenesis of skeletal parts, and an exposition of the author's own views as to the nature of the processes involved.

> The time is past when it was possible to describe the whole long and complex story of skeletogenesis in terms of the effects of mechanical stresses, whether of extrinsic and functional origin or arising in direct consequence of the developmental process itself. The form of the cartilaginous model is produced, at least in the case of the larger shafted elements, under the aegis of a growth pattern intrinsic from a very early stage within each element. Neither the functioning of muscles, nor mechanical interaction between the developing elements, can now be allowed any major rôle in the first production of form, even though the second set of factors does play an essential part in bringing about the separation of the continuous anlage into the several elements of the limbs.

The author regards the form and structure of bone as a compromise between many factors. The adaptiveness shown in bony structure to functional requirements has long been stressed in the literature of regulation. But Murray's book makes it perfectly plain that the mechanist explanation is not good enough by itself to furnish a reasonable picture of the basic biology of bone building.

THE EGGS OF MAMMALS. *Experimental Biology Monographs.*

*By Gregory Pincus. The Macmillan Co., New York.* $3.75. 8½ x 5¾; ix + 160; 1936.

This monograph gives an excellent, concise review of the experimental investigations on the growth and development of the mammalian ovum from the moment of its functional differentiation in the ovary until its implantation in the uterus. The several chapters discuss: the origin of the definitive ovum; its growth; atresia and parthenogenesis, together with a description of the intrinsic and extrinsic factors affecting the cleavage process; recently developed techniques for the experimental manipulation of living ova; activation of unfertilized eggs; and vesicle growth and implantation. A bibliography of 24 pages and author and subject indices are provided.

HOW WE CAME BY OUR BODIES.

*By Charles B. Davenport. Henry Holt and Co., New York.* $3.75. 7¾ x 5½; xii + 401; 1936.

A broad range of subjects and many facts are compressed into this little book. Its three parts cover: human ontogeny, the machinery of development—cells, genes, specialised tissues, adaptations, endocrines—the origin of life, contemporary human evolution—with an extended treatment of variation and heredity—primate relationships, and the question of the future of our species. Recent advances in genetics, organic chemistry, and physical growth are brought out. Richly illustrated with 236 photographs and charts, written in popular style, the book aims to stimulate interest in children, "as the most wonderful of natural creations."

DAS WERDEN DER TIERFORMEN. *Eine Einführung in die Grundfragen der Entwicklungsphysiologie.*

*By H.-A. Stolte. Ferdinand Enke, Stuttgart.* RM. 7. 10 x 6¼; vi + 112; 1936 (paper).

This introduction to embryology commences with a description of the cells,

chromosomes, and the process of mitosis. Examples are drawn from the familiar sea urchin, *Tubifex* and amphibian forms. Two chapters are devoted to metamorphosis, regeneration, transplantation and explanation. The concluding chapter on the underlying general principles of morphogenesis is essentially preformationist in its philosophy. The author regards as the most fundamental characteristic of living substance the universal presence in it of paired structural opposites; "colloidal particles-solute, sol-gel formation in the nucleus: achromatin-chromatin and finally the total plasma-nucleus system are such opposites. At the boundary surfaces of these pairs the essential life phenomena manifest themselves."

There is a brief bibliography and an adequate index.

ADVENTURES BEFORE BIRTH.

*By Jean Rostand. Translated by Joseph Needham. Victor Gollancz, London.* 4s. 6d. $7\frac{1}{4}$ x 5; 157; 1936.

Credit is due to the author of this book for doing well a task that many fail in,—that of presenting for lay consumption a scientific subject without the aid of many scientific terms, while at the same time giving a clear interpretation of facts. The history of the germ cells, the development of the fertilized egg, embryo and foetus are chronicled in sufficient detail to render a satisfactory panoramic view of these processes.

APPARATUS FOR THE DISSECTION AND STUDY OF EMBRYOS. *University of California Publications in Zoology, Vol. 41, No. 9.*

*By J. A. Long. University of California Press, Berkeley.* 25 cents. $10\frac{1}{4}$ x $6\frac{3}{4}$; 18; 1936 (paper).

In this apparatus electrically controlled knives (made of safety-razor blades) operate under the binocular dissecting microscope. A camera photographs the dissections stereoscopically. The text is supplemented by 3 photographic plates.

## PHYSIOLOGY AND PATHOLOGY

STARLING'S PRINCIPLES OF HUMAN PHYSIOLOGY. *Seventh Edition.*

*Edited and Revised by C. Lovatt Evans. The Chapters on the Central Nervous System and Sense Organs Revised by H. Hartridge. Lea and Febiger, Philadelphia.* $8.75. $9\frac{3}{8}$ x 6; xiii + 1096; 1936.

A TEXTBOOK OF PHYSIOLOGY *for Medical Students and Physicians. Thirteenth Edition, Thoroughly Revised.*

*By William H. Howell. W. B. Saunders Co., Philadelphia.* $7.00 net. $9\frac{1}{8}$ x $5\frac{3}{4}$; 1150 + 6 plates; 1936.

HUMAN PHYSIOLOGY. *A Text-Book for High Schools and Colleges. Seventh Edition, Revised.*

*By Percy G. Stiles. W. B. Saunders Co., Philadelphia.* $2.25. $7\frac{1}{2}$ x 5; 446; 1936.

AN ELEMENTARY MANUAL OF PHYSIOLOGY. *For Colleges, Schools of Nursing, of Physical Education, and of the Practical Arts. Fifth Edition, Revised.*

*By Russell Burton-Opitz. W. B. Saunders Co., Philadelphia.* $2.50. $7\frac{1}{2}$ x $5\frac{1}{4}$; 442; 1936.

A TEXT-BOOK OF PHYSIOLOGY. *Second Edition.*

*By H. E. Roaf. William Wood and Co., Baltimore.* $6.75. $8\frac{1}{2}$ x $5\frac{1}{4}$; viii + 679; 1936.

Starling's text has been completely revised and brought up to date by Professor Evans, Professor Hartridge revising the chapters on the nervous system and sense organs. The greatest changes and additions have been made on the chapters dealing with biochemistry. The references, rather more numerous than in most text books on physiology, are put in as footnotes and at the end of chapters.

Dr. Howell has been somewhat more conservative in revising his classic work, making many minor changes here and there but few large ones. He too has picked biochemistry as the subject requiring the greatest addition and rewriting.

Dr. Stile's text book and Dr. Burton-Opitz's text book, both being much more elementary than the two just mentioned, needed little revision since neither one goes into any detail on biochemistry or other rapidly changing fields of physi-

ology. The chapter on internal secretions has been expanded and revised in the former, while a new part on reproduction has been added in the latter.

Dr. Roaf's text, not previously reviewed in this journal, falls between the first two books and the last two books mentioned above in respect to its complexity and scope. The whole field is covered in a general sort of way but not very thoroughly and with but a few references to other text books. If a book is not intended to be profound, in compensation it should be easy to read. Dr. Roaf, in our estimation has not exactly rung either bell.

MÉTABOLISME ET OBÉSITÉ. *Publications du Travail Humain Série A. No. 7.*

*By W. Liberson. Conservatoire National des Arts et Métiers, Paris.* 25 francs. 10 x 6½; 157; 1936 (paper).

The author has obtained the basal metabolism of 688 healthy women whose only complaint was obesity. Incidentally, he notes that only about one-third of these women could really be called obese. [Reginald, the Office Boy queries as to whether these others were called luscious, or merely Mae.]

The observed relationship between basal metabolism and weight, stature and age, leads to the conclusion that stature is unnecessary in the calculation of the expected individual metabolism. He finds that Dreyer's formula, which places metabolism directly proportional to the square root of weight and inversely proportional to age, fits best his series of observations. The most significant point of this investigation appears to be that, contrary to general assumption, the expected or standard metabolism of obese women can be calculated directly from the general equations used for the calculation of the metabolism of the non-obese.

The author has carefully and critically reviewed the literature and although he seems to miss at times the significance of his statistical results, this monograph should stimulate others to base standards, such as these, on a sufficiently large number of individuals. The bibliography is fairly complete.

BIOLOGISCHE WIRKUNGEN DES ALKOHOLS AUF DEN STOFFWECHSEL. *Ergebnisse experimenteller Untersuchungen in dem von mir geleiteten Laboratorium unter Verwendung der Widmarkschen Mikromethode der Blutalkoholbestimmung über Wirkungen des Alkohols auf den Stoffwechsel, über die Verwertung des Alkohols bei der Muskelarbeit, über Beziehungen des Blutalkoholgehaltes zum Rauschzustande, über Insulinbehandlung der Alkoholintoxikation und die Insulinblockade der Ganglienzellen.*

*By Adolf Bickel. Georg Thieme, Leipzig.* .90 marks. 9 x 6; 31; 1936 (paper).

This volume discusses the results of experiments on (1) the effect of alcohol on metabolism, (2) the utilization of alcohol during muscular activity, (3) the relation of a state of intoxication and alcoholic content of the blood, (4) the treatment of alcohol intoxication with insulin, and (5) the insulin blockade of the ganglion cells. The author finds that frequently, though not uniformly there is a rise in basal metabolism after consumption of alcohol. During muscular activity alcohol is oxydized in considerable quantity, but however without constituting the source of muscular strength, as is shown from the decrease of the alimentary alcohol curve occurring with a simultaneous rise of the respiratory quotient above its alcohol value. The alcohol enables the carbohydrate supply of the body to stretch farther, and thereby promotes indirectly the revictualing of the muscles with sugar for their work. The normal supply of insulin in the body is without demonstrable effect on the metabolism of alcohol in the blood arising from alcoholic consumption. An increased blood sugar content seems to hinder the development of a state of intoxication. Subcutaneous injections of insulin bring about a decrease in the amount of blood sugar and alcohol in the blood. Insulin shortens the duration of a state of intoxication. This phenomena is brought about by what the author calls the insulin blockade of the ganglion cells.

THE VEGETATIVE NERVOUS SYSTEM. *A Clinical Study.*

*By Wulf Sachs. Introduction by Walter*

*L. Brown. Cassell and Co., London.* 15s. net. 8½ x 5½; x + 168 + 8 plates; 1936.
The book is an attempt to give the reader a quick orientation in and a practical approach to the whole question of vegetative imbalance; would give, in other words, an ordinary description of the various tests, clinical observations, diagnostic criteria and the essential anatomical, physiological, and pharmacological data.

The author shows that, on the anatomical side, the principal additions to our knowledge of the vegetative nervous system since the pioneer discoveries of Gaskell and of Langley have been in relation to the ties between it and the central nervous system. Emphasis is placed on the important influence of the pre-existing state of tonus in the vegetative nervous system on the results obtained by subsequently stimulating it. A case record is reproduced in the appendix to demonstrate the scheme according to which all examinations were carried out. Fifty different tests were applied to each patient. The bibliography cites the most important references. A subject and an author index have been added.

VITALITY AND ENERGY IN RELATION TO THE CONSTITUTION.

*By T. E. Hammond. H. K. Lewis and Co., London.* 12s. 6d. 8½ x 5½; xii + 314; 1936.
Whenever the physician really becomes interested in treating the patient and not the disease alone, he becomes conscious of the existence of something, an *X*, which differentiates the patient who recovers from the one who succumbs to an infection. This unknown factor is by Hammond called the vitality of the individual. The term is never clearly defined but is stated as being akin to muscular tonus and its centre is supposed to be somewhere at the base of the brain. Although his concepts are criticizable and the terminology is vague, the main point of this work cannot be disregarded. It is that therapy to be efficacious must be based on knowledge of the individual constitution and its reaction to disease. The book is written in an engaging style and is replete with pertinent observations drawn from the author's long clinical experience. Especially interesting is the fact that his philosophy of constitution is drawn from the teachings of such great clinicians as Paget and Hutchinson. There is no bibliography and no mention is made of the recent vigorous revival of constitutional studies.

DENTAL SURVEY OF SCHOOL CHILDREN, AGES 6–14 YEARS MADE IN 1933–34 IN 26 STATES. *Public Health Bulletin No. 226. By C. T. Messner, W. M. Gafafer, F. C. Cady and H. T. Dean. U. S. Government Printing Office, Washington.* 20 cents. 9 x 6; vi + 248; 1936 (paper).
Almost one and a half million children were examined in a dental survey conducted by the United States Public Health Service in 1933–34. In this bulletin are presented all the data collected regarding the oral conditions and needs of the children. Separate tabulations have been made for each county of each of the 26 states. There is no general summary of the results nor have any conclusions been drawn from the mass of information obtained. It is probable that the chief reason for this uncommon reticence is that the dental examinations were conducted by a number of individuals and the technique of examination and of recording was not uniform. Just what was achieved by the survey is not clear to this reviewer. There is no bibliography. The reader is forewarned that the bulletin consists of 228 pages solidly filled with tables and 20 pages of text and tables.

CONTRACEPTION AS A THERAPEUTIC MEASURE.

*By Bessie L. Moses. The Williams and Wilkins Co., Baltimore.* $1.00. 7¼ x 4¾; xiii + 106; 1936.
The Bureau for Contraceptive Advice in Baltimore was established to determine the effectiveness of currently advocated methods of birth control. It was in operation for five years and collected more than one thousand cases which are studied and analyzed in this book. Information concerning color, mental status,

previous use of contraceptives, number of abortions and type of contraceptive prescribed is given. The value of the Bureau's work is shown in the fact that 85 percent of the patients were unsuccessful in their use of contraceptives previously, while after treatment and advice the method failed in only 2.71 percent of the total number of cases. The improvement in the mental and physical health of the patients was marked. There is a good index and bibliography.

PRINCIPLES AND FOIBLES OF CANCER RESEARCH. *In regard to Etiology and Nature. By William Rienhoff, Sr. Waverly Press, Baltimore.* $2.50. 9 x 5¾; vii + 200; 1936 (paper).

In this scholarly discussion of the etiology of cancer the author reviews critically although briefly a good proportion of the vast literature on the subject. His purpose is to clear "the overgrown field of investigation from luxuriant weeds and thereby admitting air and light to nurslings that have been looked for so long in vain because of their smothered existence, . . . ." These nurslings turn out to be the alleged proofs of the existence of cancer virus. In the author's opinion, the problem of the etiology and treatment of cancer can only be solved by intensive research on filterable viruses. All other theories and modes of investigation are to be discarded forthwith. The author may be entirely correct in his beliefs, but full acceptance of this or of any other theory depends on factual demonstrations and not on verbalism.

There is an extensive bibliography of some 27 pages.

LES AVORTEMENTS MORTELS. *By H. Mondor. Masson et Cie, Paris.* 65 francs. 10 x 6½; 445; 1936 (paper).

This book, combining the results of a study of the world literature on the subject and the author's own extensive experience, is practically an encyclopaedia of all the harmful effects that might possibly result from abortion. Among the more important aspects of fatal abortion dealt with are: sudden death (especially from embolus); perforation of the uterus; intrauterine instillations; infarction, gangrene, and abscess of the uterus; postabortal peritonitis, septicemia and hemorrhage; attempts at abortion in the presence of an ectopic gestation; and attempts at curettage of the uterus in women who are not pregnant. Considerable space is devoted to treatment. The numerous case histories and illustrations are chiefly from the author's material. The book is adequately documented and contains an author index arranged by the chapter headings. This is an invaluable and unique reference work in its field.

ALLGEMEINE SINNESPHYSIOLOGIE. *Stellung und Bedeutung des sinnesphysiologischen Versuches im Bereich der Observation, des exakten Experimentes und der Begriffsbildung. By Yrjö Renquist-Reenpää. Julius Springer, Vienna.* RM. 12. 9¼ x 6½; v + 160; 1936 (paper).

This is a somewhat abstruse work on the importance of the senses in the fields of scientific observation, experimentation, and thinking. The author struggles with the fact that all observations depending on the senses for determination are necessarily bound to involve error, and he believes therefore that attempts should be made to get at the fundamental factors in the physiology of the senses. He discusses his material from the physical, chemical, mathematical, and physiological viewpoints, and deduces a great many equations of extremely complex nature to show the sensitivity of the skin, fingertips, taste receptors, etc. to various stimuli. The attempt is praiseworthy but the results are so highly complex that it is doubtful that this work will prove practically useful.

TISSUE IMMUNITY. *By Reuben L. Kahn. Charles C Thomas, Springfield, Ill.* $7.50. 9 x 6; xix + 707; 1936.

An important treatise. The author presents as a basis for the work his own numerous studies, and correlates the re-

sults with manifestations of infection and immunity noted in the clinic. He states that "although the experiments in this volume have been directed mainly toward the enlarging of our understanding of the laws governing immunity to protein and microbial antigens, actually these experiments throw light on the mechanisms of different infectious diseases, and of many immunologic phenomena, whether advantageous or antagonistic to the host, constantly noted in the clinic." The experiments indicate that "tissues in immunity are defensive and not hypersensitive" and the question is raised whether in the light of newer immunologic knowledge the concept of tissue hypersensitiveness is tenable.

The author has introduced the plan of putting only the graphs in the text and the tabular matter at the end of the chapters. The volume contains a bibliography and an excellent index. We recommend this to clinicians, and students and investigators, including those in the field of parasitism, as a highly useful and stimulating book.

DIGESTION AND HEALTH.

*By Walter B. Cannon. W. W. Norton and Co., New York.* $2.00. 8 x 5¼; 160; 1936.

This is a rearrangement of the material presented by Dr. Cannon in a series of lectures commemorating William Beaumont whose experiments are the basis of the modern study of the physiology of digestion.

The book takes up the ways in which appetite, hunger, thirst, general health, and emotional excitement affect the digestive process. The description of the method used to record gastric hunger contractions and the discussion of the effect of the extrinsic nerves of the alimentary tract on the tonicity of the gastric masculature will interest and enlighten the general reader. The material is so clearly presented that it should be comprehensible to everyone. The inclusion of numerous graphs and drawings is of great value. There is a bibliography at the end of each chapter and a good index.

HORMONE DES OVARIUMS UND DES HYPOPHYSENVORDERLAPPENS. *Untersuchungen zur Biologie und Klinik der Weiblichen Genitalfunktion. Mit einen Anhang: Hormonale Schwangerschaftsreaktion. Hormon des Hypophysenzwichenlappens. Zweite vermehrte Auflage.*

*By Bernhard Zondek. Julius Springer, Berlin.* RM. 58 (paper); RM. 59.80 (cloth). 9¾ x 6¼; xi + 638; 1935.

Since the first edition of this book appeared in 1931 the author has continued his research on the hormones of the ovary and hypophysis and their effects on the sexual apparatus and reproductive functions of the female. These further investigations are incorporated in this second edition, with the result that it is almost twice the size of the original. The additions include fifteen new chapters, chiefly on prolan, 56 new illustrations and changes or additions to the other chapters. The book is documented but a systematic bibliography has not been provided. There is a subject index.

EXPERIMENTAL STUDIES ON A TRANSMISSIBLE MYELOMATOSIS (RETICULOSIS) IN MICE. *Acta Radiologia Supplementum XXIX.*

*By Otto Kaalund-Jørgensen. Levin and Munksgaard, Copenhagen.* Dan. Kr. 2. 10½ x 7¾; 142 + 15 plates; 1936 (paper).

The author finds: (1) that myelomatosis is readily transmitted by apparently metastasis-free brain tissue; (2) that myelomatosis can be transmitted by tumor material diluted 1 to 400,000 (equal to each mouse getting about 8 cells); (3) that myelomatosis cannot be transmitted even to pre-irradiated mice by positively cell free material; (4) that myelomatosis can be transmitted by other metastasis-free organs, and by non-leukotic blood. The experimental technique is described in detail, and at all stages the evidence is checked against the findings of previous investigators. The data are treated statistically throughout. There is an excellent bibliography, and, in addition, fifteen plates with explanatory notes accompanying each figure.

LANE MEDICAL LECTURES: STUDIES IN CARDIOVASCULAR REGULATION.

*By G. V. Anrep. Stanford University Press, Stanford University.* $1.50 (paper); $2.25 (cloth). 10 x 7; 118 + 14 plates; 1936.

In the five lectures here reported the author discusses certain aspects of the experimental work done in the field of circulatory physiology. He mentions mostly his own experiments and those of his colleagues and assistants and refers to those of other physiologists only when their findings tend to confirm, supplement or contradict his own. The subjects discussed concern the proprioceptive mechanism of cardiovascular regulations, the respiratory regulation of the heart rate, the dynamics of coronary circulation, the coronary blood flow, the blood flow through skeletal and plain muscles. The experiments and technique are described in great detail and illustrated by drawings and photographs.

MENSTRUATIONSSTÖRUNGEN HORMONALEN URSPRUNGS. *Eine Klinische Untersuchung.*

*By P. N. Damm. Levin and Munksgaard, Copenhagen.* 12 kroner. 9½ x 6½; 285; 1936 (paper).

This is a presentation of the results and methodology of experimental work conducted by the author in the Woman's Clinic of the University, Copenhagen, on the cyclical changes in the ovary and uterus; analyses of the follicular hormone; the nature of the gonadotropic hormone; and hormonal effects on menstruation and its disorders. Thirty-seven typical case histories are included. The significance of the work is largely clinical and its chief interest will be to gynecologists. The bibliography, 19 pages in length, contains only those titles consulted by the author during his investigations. An index is lacking.

FOOD, FITNESS AND FIGURE.

*By Jacob Buckstein. Introduction by Harlow Brooks. Emerson Books, New York.* $2.00. 8 x 5¼; xii + 252, 1936.

This is not a faddist book. It is written to give the layman an accurate picture of what is known about foods and their properties at the present time and to show how to apply this knowledge to an intelligent diet. The most important points to be kept in mind are emphasized by reiteration in various chapters, as, for example, the great importance of milk in the diet. An appendix supplies diet lists for reducing, gaining, or maintaining weight; a table of 100 calory portions of the more common foods; a set of charts showing common foods as sources of vitamins and food value as to amount of protein, calcium, phosphorus, iron and copper; standard weight charts for age and height of men and women. There is an index.

PREVENTIVE MEDICINE. *Fifth Edition, Reset.*

*By Mark F. Boyd. W. B. Saunders Co., Philadelphia.* $4.50. 9⅛ x 5¾; 561 + 1 folding map; 1936.

The revision of this fifth edition of a well known text includes changes or new material on colds, ringworm, psittacosis, diphtheria, encephalitis, poliomyelitis, pneumonia, tuberculosis, malaria, typhus and relapsing fever. Recent developments in sewage treatment are noted and sections are added dealing with mottled enamel, and with the rat from a public health standpoint. The sections on vitamins, silicosis and general morbidity incidence have been enlarged.

DIABÈTE ET CHIRURGIE.

*By H. Chabanier and C. Lobo-Onell, with the collaboration of E. Lelu. Masson et Cie, Paris.* 22 francs. 7¾ x 5¼; xii + 168; 1936 (paper).

The presence of diabetes complicates the dangers of operative procedure, increases the liability to gangrene, septicemia and other infections, and adds the risks associated with coma and precoma states. The authors emphasize these dangers and outline their pre-operative treatment devised to reduce to a minimum the operative risk and post-operative toxic accidents in this group of patients. The book is chiefly of interest to surgeons.

S. O. S. Pour la Défense de la Race. *By Arthur Vernes. Préface by Alexis Carrel. Librairie Maloine, Paris.* 6½ x 5; 62 + 1 folding chart; 1935 (paper).
Dr. Vernes is intent upon scaring us to death with his hair-raising story of the insidious enemy on the way to bring our civilization to ruin, as he says. We agree that syphilis is a dreadful disease, and perhaps a bit of exaggeration about it isn't going to do any harm. May the good doctor have success in his venture!

Diseases and Parasites of Poultry. *U. S. Department of Agriculture Farmers' Bulletin No. 1652.*
*By John S. Buckley, Hubert Bunyea and Eloise B. Cram. U. S. Department of Agriculture, Washington.* 10 cents. 9¼ x 6; ii + 70; 1936 (paper).
The cause and nature of each disease is outlined, and what is known about prevention and treatment. In the case of parasites the life history of the parasitic animal is included.

## BIOCHEMISTRY

Colloid Chemistry of Cellulosic Materials. *U. S. Department of Agriculture Miscellaneous Publication No. 240.*
*By Alfred J. Stamm. U. S. Government Printing Office, Washington.* 10 cents. 9⅛ x 6; 91: 1936 (paper).
The author correlates all the information and hitherto isolated facts contained in what he feels to be the most important papers so far published in this field. The appendix includes the following tables: (1) Refractive indexes and double refractions of cellulose fibres, (2) fibre contraction, (3) dimensions of unit cell of cellulose, (4) time for complete development of cellulose fibres, (5) size and structure of units of wood, (6) pore radii of membranes, (7) tensile strength of various fibres, (8) particle size, molecular weight, and molecular dimensions of cellulose, (9) electrical conductivity, (10) potential of cellulosic materials against distilled water, (11) sorption of gases by cellulosic materials, (12) absorption of polar gases and vapors by cellulosic materials, (13) various equations and symbols. The bibliography lists 280 papers.

Le Venin des Araignées. *Monographies de l'Institut Pasteur.*
*By J. Vellard. Masson et Cie, Paris.* 45 francs. 10 x 6½; 311; 1936 (paper).
The author has done extensive work over a period of years at the Antivenom Institute of Butantan, São Paulo, on the physiological and chemical properties of spider venom. Naturally, in this book, the arachnids of South America receive the greatest consideration and space, although species of other parts of the world are discussed to some extent—especially those which are dangerous to man. The number of species whose bite is fatal or seriously dangerous is, the author claims, relatively small. Incidentally, he corroborates the statements that the toxicity of the Black Widow (*Latrodextus mactans*) which recently invaded parts of the United States has been greatly over-rated.
The treatment and vaccine sera listed and discussed in the final section are specific for Brazilian species. A bibliography of over 200 titles is included but there is no index.

Gefässerweiternde Stoffe der Gewebe. *By J. H. Gaddum. Introduction by H. H. Dale. Georg Thieme, Leipzig.* RM. 18 (paper); RM. 20 (cloth). 9¾ x 6½; xii + 200; 1936.
This is a complete and comprehensive survey of all the recent work done in the branch of pharmacology which deals with the effect on vascular dilation of tissue substances. The author deals at great length with the physiological effect on the vascular system of histamine, azetylcholin, and adenosin. There is a section on tissue extracts of unknown constitution, dealing with the effect of extracts of mammalian blood on the various organs, and extracts from other organs, such as heart, liver, spleen, pancreas, intestine, brain, gonads, eyes, urine, etc. The book is translated from the English. It contains 28 pages of bibliography and an

index. A useful and valuable reference book.

LA CHIMIE DES FERMENTATIONS.

*By Marc H. Van Laer. Masson et Cie, Paris.* 75 francs. $10\frac{1}{8}$ x $6\frac{5}{8}$; 350; 1936 (paper).

This is a good systematic treatise presenting the fundamental facts and theories concerning the properties and behavior of the enzymes, sugars, amino acids, bacterial metabolism, etc., that the French technical chemist should know for a fuller understanding of the phenomena of fermentation. The bibliography seems to us inadequate (21 titles), and a detailed table of contents substitutes for an index.

BIOCHEMICAL AND ALLIED RESEARCH IN INDIA IN 1935.

*Society of Biological Chemists, Bangalore, India.* $8\frac{1}{2}$ x $5\frac{1}{2}$; 130; 1936 (paper).

## SEX

THE FUTURE OF MARRIAGE IN WESTERN CIVILISATION.

*By Edward Westermarck. Macmillan Company, New York.* $2.50. $8\frac{1}{2}$ x $5\frac{3}{4}$; xiv + 281; 1936.

This eminent authority on the history of human marriage takes issue with the proponents of the theory that in the occidental countries the institution of marriage is facing a crisis, bankruptcy and collapse, and that free love will take its place. He briefly restates his viewpoints regarding the essential elements of marriage and then proceeds to discuss those factors which are regarded as important causes of marital unhappiness. In this respect, he utilizes well-known observations regarding sexual maladjustment, adultery and jealousy, social and economic difficulties, the ages of the partners, desire for children and so on. These observations show that increasing knowledge, forethought and self-control would increase happiness in marriage. The author also outlines the practical consequences of trial, companionate and temporary marriage, concubinage and free love, and gives a number of reasons to show that these forms cannot become universal, at least under the present social and legal regulations. In conclusion, Westermarck expresses the belief that the institution of marriage will persist because

> the unity of sensual and spiritual elements in sexual love, leading to a more or less durable community of life in a common home, and the desire for and love of off-spring, are factors which will remain lasting obstacles to the extinction of marriage and the collapse of the family, because they are too deeply rooted in human nature to fade away, and can find adequate satisfaction only in some form of marriage and the family founded upon it.

This book also includes chapters dealing with such topics as monogamy and polygamy, divorce and sexual behaviour and morality. This last chapter is expecially praiseworthy and justifies the author's great reputation. While the facts and the theories presented in this volume have already been expressed by the author himself and by others, the clarity of style and the able summary of the facts places this book among the first on the required reading list of all cultured persons.

CHANGE OF LIFE IN MEN AND WOMEN.

*By Marie C. Stopes. G. P. Putnam's Sons, New York.* $2.00. $7\frac{1}{2}$ x $5\frac{1}{4}$; x + 239; 1936.

Once again Dr. Stopes attempts to enlighten us on a perplexing subject. In this treatise she denounces numerous books already written upon the menopause because she says they emphasize in too lurid pictures the alleged inevitable miseries that women are to anticipate at this "crisis." But unfortunately for the scientific force of the *opus* she quotes with manifest approval such hot passages as accord with her own pet prejudices. Thus we are told about the "sensual puberty" of the climacteric that is alleged to create "a fresh spring-time, and bring about a second youth." My eye—or rather our collective and bilious editorial eye!

It is urged throughout that there is a climacteric in the male. In any precise scientific sense this is not true. Progressive senescence in the male involves the

reproductive organs as well as all others, but there is biologically nothing in the male that is justly comparable with the sharp cessation of ovulation at the menopause of the female.

In conclusion, it is stated, justly enough, that the change of life may be and should be no more a "crisis" than is weaning or adolescence, and that, with proper care and understanding, lives after the change, "should be better balanced and capable of even richer and fuller experience than ever before."

L'Esthétique Mammaire à Travers l'Histoire.

*By C. Claoué and I. Bernard. Librairie Maloine, Paris.* 10 x 6½; 24; 1936 (paper).

An extremely brief history of the esthetic concepts of the human female breast as depicted in painting and sculpture from prehistoric times to the present. The authors assert (as any salesgirl or designer of women's clothes knows) that there is now a swing-back from the boyish, immature type idealized in the post-war period to the more classical plumpness. The pamphlet is illustrated.

## BIOMETRY

Statistical Methods in Biology, Medicine and Psychology. *Fourth Edition, Completely Revised.*

*By C. B. Davenport and Merle P. Ekas. John Wiley and Sons, New York; Chapman and Hall, London.* $2.75. 7 x 4⅞; xii + 216; 1936.

This edition of a hand book which has had great popularity ever since its first publication in 1899 remains essentially the same as the previous editions, except that a number of the symbols have been changed to conform to A. K. Kurtz and H. A. Edgerton's suggestions for a standardized usage.

Some pages have been added on the following subjects: analysis of variance, extension of the theory of small samples, multiple and partial correlation, and some applications of statistics to economics. Also a number of extra tables have been added.

Statistics for Students of Psychology and Education.

*By Herbert Sorenson. McGraw-Hill Book Co., New York.* $3.50. 9 x 5¾; viii + 373; 1936.

This textbook is clearly written and comprehensive enough to give the student without mathematical training a fairly accurate knowledge of the elementary methods of statistical analysis. The subject matter includes the methods of calculating and discussion of the centering constants, measures of variability, normal curve, correlation coefficients, and probable errors. As is usual in most elementary textbooks, the limits of applicability of the correlation coefficients are not adequately emphasized. Numerous examples are given to illustrate the use and calculation of the constants. A selected list of textbooks is furnished and there is an appendix with tables of squares and square roots, areas and ordinates of the normal curve, and probability of a given deviation occurring in a normal distribution.

Examples in Finite Differences, Calculus and Probability. *Supplement to an Elementary Treatise on Actuarial Mathematics.*

*By Harry Freeman. The University Press, Cambridge; Macmillan Company, New York.* $2.50. 8½ x 5½; 86; 1936.

This is a supplement to the author's well-known textbook on actuarial mathematics. It contains 400 problems and their answers. In addition, there is a section containing helpful hints on the method of solving the problems. It is difficult to appraise the clearness of the questions and the accuracy of the answers without actually solving all the problems. A casual survey shows that in general they are interesting and stimulating. However, it seems that at times the author has taken more pains to make the questions difficult to understand rather than they intrinsically are. Notwithstanding, this is an indispensable addition to the student's library.

Calcul des Probabilités.

*By J.-B. Pomey. Gauthier-Villars, Paris.* 25 francs. 9¾ x 6½; 85; 1936 (paper).

This outline of the theory of probability is designed especially for students of engineering. After an exposition of the elementary theorems, the author discusses the geometric interpretation of probabilities and proceeds to illustrate the theory with applications to problems in telephone traffic, kinetic theory of gases, Brownian movements, *et similia*. The order of presentation is not of the best from the pedagogic standpoint and it is evident that the book proposes to give only a very general idea of the subject.

## PSYCHOLOGY AND BEHAVIOR

MECHANISTIC BIOLOGY AND ANIMAL BEHAVIOUR.

*By Theodore H. Savory. Watts and Co., London.* 7s. 6d. 7¾ x 5¼; xv + 182; 1936.

In the words of the author, this book contains "a study of sub-intelligent behaviour in animals, and especially among invertebrates." The bulk of the volume is concerned with descriptions of reflexes and reflex actions, tropisms and tropistic behavior, and instinct. It then proceeds to show how habit and evolution are related to these actions that are interpreted as purely mechanistic. The points made are frequently illustrated from the author's study of spiders.

Although the author himself calls this a "study of sub-intelligent behaviour," he rather fails to give due weight to the fact that the term "sub-intelligent" action implies the existence of the "intelligent" action. His argument appears in the end to lead to the conclusion that the only behavior we really know anything about is that comprised within the pure reflexes and tropisms of the invertebrates, and that at best it is only a good working hypothesis that there may be intelligence involved in the behavior of higher animals—surely an odd conclusion for a primate to reach.

Finally, in spite of the fact that the preface gives the impression that breadth of view will be found in this study, we find, especially in the concluding summary, rather sweepingly dogmatic statements concerning the absurdity of vitalism and the incontrovertible rightness of the mechanist interpretation of vital phenomena. The book contains both index and bibliography.

THE PSYCHOLOGY OF FEELING AND EMOTION.

*By Christian A. Ruckmick. McGraw-Hill Book Co., New York.* $4.50. 9 x 6; xiii + 529; 1936.

A comprehensive textbook in a field of psychology where there has been a need for an up-to-date text. The treatment leans heavily on quotation from original sources, but is not particularly strong on the side of constructive, critical synthesis. It is of some interest to note that 44 pages are given over to "The Facial Expression of Emotion," whereas the work of Freud and all of his followers is disposed of in 14. The chapter on "The Rôle of Feeling and Emotion in Psychoanalysis" closes with the following paragraph:

> We might also question the assumption of subtlety on the part of so primitive and blind an entity as the id, or even the ego. The whole system, we recall, is a "feeling psychology." But the subterfuges that appear in hiding the real character of the libido are worthy of the astuteness and cleverness of a supreme order of intellect. How is it possible, then, to get the process of camouflaging started—a process that takes the acute mind and long practice of an analyst to unravel? If Freud has ever satisfactorily answered this fundamental question, the writer is unaware of it. To imply it in the general order of nature is probably too much for a present-day naturalist!

At the close of the volume the author gives his own views on education and culture. It occurs to us as possible that the work might have been better if he hadn't. There is an index, and at the end of each chapter a summary and a list of review questions. The wise and discreet teacher will be able to use this book effectively in classroom work.

PRINCIPLES OF TOPOLOGICAL PSYCHOLOGY.

*By Kurt Lewin. Translated by Fritz Heider and Grace M. Heider. McGraw-Hill Book Co., New York.* $2.50. 9 x 6; xv + 231; 1936.

The author of this book believes that "psychology needs concepts which can be

applied not merely to the facts of a single field like child psychology, animal psychology, or psychopathology, but which are equally applicable to all of them." He studied topology (the mathematics of space relationships) and made use of its concepts, "which soon appeared . . . particularly fitted to the specific problems of psychology." In this volume, therefore, he has attempted to develop a group of concepts that may be used as a framework for integrating all of the various branches of psychology. The goal of this method of attack is to discover laws that will enable the prediction of individual cases. These laws must consider the relationship between the individual and the situation, an impossibility with only our "averages" and "statistical characterizations."

Here we have a new idea, a new method in the field of psychology. The author is undoubtedly an idealist; but whether he is on the wrong track or whether he is the creator of a revolutionary point of view will in the fullness of time become clear. The book is interesting because of its novelty. The material, however, is presented in an extremely technical form. Both index and bibliography are included.

ANTICIPATORY RESPONSES IN SERIAL LEARNING BY CHIMPANZEE. *Comparative Psychology Monographs, Volume 13, Number 2. By S. D. S. Spragg. The Johns Hopkins Press, Baltimore.* $1.25. 10 x 6¾; 72; 1936 (paper).

"Certain acts tend to anticipate the stimuli which previously initiated them, upon repetition of the situation in which the originally adequate stimuli occurred. Such responses are frequently designated 'anticipatory responses' . . . . As descriptive of organic behavior the term need make no assumptions regarding any foresight, expectancy, or prevision on the part of the organism. . . ." A series of stylus maze experiments, with five chimpanzees as subjects, demonstrated that extended training reduces all anticipatory responses. The author states that the cues for the solution of a maze problem when the extra-organic stimuli offered the animal at each choice point are presumably identical "may depend upon a summational effect from having made a certain number of choice responses of the same sort, or else upon some still more subtle type of cue which we tentatively designate as *symbolic*." The summational effect here referred to cannot be entirely disassociated from *foresight*, *expectancy*, or *prevision*, and it seems to us that "symbolic" is being used somewhat in the sense that others use "conditioned."

STUDIES IN INFANT BEHAVIOR III. *University of Iowa Studies in Child Welfare, Vol. 12, No. 1.*

*By M. A. Wenger, Josephine M. Smith, Charles Hazard and Orvis C. Irwin. University of Iowa, Iowa City.* $1.00 (paper); $1.35 (cloth). 9⅛ x 6⅛; 207; 1936.

This bulletin contains five reports which treat respectively of (1) conditioned responses in newborn infants; (2) brightness values of red, green and blue in the newborn; (3) skin resistance of infants; (4) age and reflex conduction rate in the patellar reflex; (5) changes in vertebral reactions pattern during infancy. Each of the papers demonstrate a careful experimental technique but in none are the results very definite since they are derived from too few observations. Of particular interest appear to be the first two papers. In one, by M. A. Wenger, it is asked whether or not newborn infants can be conditioned. The author's observations show that apparently in some infants this is possible although the reactions are unstable. In the second paper, J. W. Smith reports her experiments on the relative appreciation of color brightness by the newborn. From her findings it appears that the reactions of male infants correspond to those of adults with total color-blindness, while those of the females are similar to those of adults who do not see red. The author explains these factors by postulating a theory of ontogenetic development of color vision. An adequate bibliography accompanies each article.

STUDIES OF CEREBRAL FUNCTION IN PRIMATES. *Comparative Psychology Monographs, Vol. 13, No. 3. Serial No. 63.*

*By C. F. Jacobsen, with the coöperation of J. H. Elder and G. M. Haslerud. Johns Hopkins Press, Baltimore.* $1.25. 10 x 6¾; 68; 1936 (paper).

Monkeys were trained in two types of behavioral tests for (a) simple or immediate responses and visual discrimination and (b) delayed responses. Experimental lesions were then made in various areas of the cerebral cortex and the tests repeated. A summary of the results of these experiments show: (1) Unilateral lesions caused no impairment of performance on any tests. There was no evidence of hemispheral dominance. (2) Complete bilateral lesions of the frontal association areas caused loss of ability to perform delayed responses, and subtotal lesions caused a shortening of the time through which memory was effective. Memory for simple response habits and visual discrimination habits was not impaired either with partial or complete lesion, nor was the ability to learn new tasks of a similar nature reduced. (3) Lesions in other cortical areas produced slight, if any changes.

RESEARCH IN DEMENTIA PRECOX. (*Past Attainments, Present Trends and Future Possibilities.*)

*By Nolan D. C. Lewis. The National Committee for Mental Hygiene, New York.* $1.50. 9 x 6; xi + 320; 1936.

A general survey of the various aspects of psychiatric research with their particular applications to the "dementia precox" problem. The author, professor of neurology in Columbia University and an associate of various research institutions, has produced a dependable book. No attempt is made to present a thoroughgoing critical analysis of achievements that have been made, but some general principles are offered as well as a few specific leads for future procedure. Repeatedly does Dr. Lewis emphasize the need for greater precision in methods and for coördination of what is known and discovered. Each chapter is thoroughly documented, but the usefulness of the volume is impaired by the lack of an index.

ABNORMAL PERSONALITY AND TIME.

*By Nathan Israeli. With a Foreword by Irving J. Sands. Introduction by Gardner Murphy. Science Press Printing Co., Lancaster.* $2.50. 9 x 6; 123; 1936.

The author believes that in diagnosis of psychosis great benefit is derived from the observation of the patient's future outlook. He introduces a method of evaluating this by having the patients write future autobiographies. Examples are given of future autobiographies written by mental patients and adolescents with high I.Q.s. The technique is interesting and once the analysis of results can be based on precise definitions it should permit some important contributions to one aspect of human biology which has not received adequate treatment. There is a fairly complete bibliography.

## DE OMNIBUS REBUS ET QUIBUSDEM ALIIS

PHILOSOPHY AND THE CONCEPTS OF MODERN SCIENCE.

*By Oliver L. Reiser. The Macmillan Company, New York.* $3.50. 8½ x 5¾; xvii + 323. 1935.

This scholarly work is an apology for humanism. As few terms have been more abused than this the author feels constrained to explain that he uses it in the sense originated by the philosophers of the reformation. Yet there is a great difference between the humanism of Erasmus and that of his Italian contemporaries, and it is inevitable that a philosophy based on the concepts of modern science should differ widely from both, since during the past few centuries the changes in our scientific concepts have been as great as in our apparel.

The first part of the book is devoted to a discussion of the physical sciences, and indicates a thorough familiarity with the works of such modern writers as Korzybski, Schroedinger, Bertrand Russell, Heisenberg, Einstein, Eddington, Whitehead, etc. From their writings Reiser draws heavily, and to their conclusions adds many of his own. Perhaps the most startling of these is his synthesis of relativity and absolutivity of motion. Growth he believes to be a form of motion which

is not relative, since it is always in the direction of the increasing complexity of parts. (But how about the growth of *Sacculina* or *Entoconcha*, or the pupation period of the Diptera?) Also, since the theory of relativity in its most general form involves the curvature of space in a fourth dimension, to make room for absolute motion we need only postulate a fifth dimension in which the laws of relativity do not hold. This fifth dimension he finds in consciousness. But since Einstein has postulated ten dimensions, and as many as 137 have been credited to Eddington, it is difficult to see how Dr. Reiser finds a paltry five adequate for his philosophy.

The second part of the book deals with the social sciences. It is about half the length of the first part, not because the social sciences are less significant, but because being younger their contribution to human learning does not as yet bulk so large. The climax of this part comes with the author's identification of emergent evolution with *Gestalt* psychology, on the ground that in each case the whole is greater than the sum of its component parts.

There is an index of six pages, and the book is completely documented throughout. It is made hard reading by the use of unusual words, such as fixate, labile, appetition, nisus, which are not found in ordinary desk dictionaries.

THE GREAT CHAIN OF BEING. *A Study of the History of an Idea. The William James Lectures Delivered at Harvard University, 1933.*

*By Arthur O. Lovejoy. Harvard University Press, Cambridge.* $4.00. 8¾ x 6; xii + 382; 1936.

This book is based on the second series of the William James Lectures on Philosophy at Harvard University which were given by Professor Lovejoy. He has, however, expanded these lectures somewhat, chiefly "by the addition of more citations of illustrative passages." The lectures trace the history of the idea of "being" from Plato through various periods of thought, such as mediaeval, eighteenth century, and Romantic, to contemporary thought. The author has attempted to show how this idea influenced not only the field of philosophy, but also other fields of thought such as religion, literature, and biology. And it is here that the "citations of illustrative passages" add interest as well as enlightenment. In the first lecture, the author says that the "Great Chain of Being" is a concept consisting of three ideas. He makes no statement as to just what those ideas are, but seems to agree with Leibniz for whom "the essential characteristics of the universe are . . . plentitude, continuity, and linear gradation."

Professor Lovejoy has not expounded his own nor any other single philosophy. His material has been drawn from many sources and, in his own words, the resultant study "aims at interpretation and unification and seeks to correlate things which often are not on the surface connected." The lectures were designed for a "mixed academic audience" rather than for specialists and the phrasing is, possibly for that very reason, remarkably uninvolved. The book contains no bibliography but there are notes (in an appendix) and an index.

THE RETREAT FROM REASON. *Conway Memorial Lecture delivered at Conway Hall May 20, 1936. Introduction by Julian Huxley.*

*By Lancelot Hogben. Watts and Co., London.* One shilling net. 6½ x 4; xi + 82; 1936 (paper).

In this book Professor Hogben plumbs the depths of pessimism. That all is not well with contemporary civilization no sane person will deny, but few will follow him so far as to assert that it is without any salvage value whatsoever. To be sure, Professor Hogben does not state this in so many words, but anyone who wades through his lengthy Jeremiad is likely to get the impression that he believes it. His vituperations are directed chiefly against modern education. The function of education he believes to be the acquisition, dissemination, and application of knowledge and the greatest of these is application—"knowledge for use" he calls

it. Yet in actual practice we find that we are acquiring and disseminating knowledge faster than we learn to apply it, with the result that positions of political responsibility are everywhere occupied by persons who know everything about how a state should be governed and nothing about how to govern one. The countries of southern and central Europe afford Professor Hogben much material in support of his theory, but he is not unmindful of the situation in England, which is not above criticism, and the vigor with which he attacks his own as well as the adjacent countries is like that of the shepherd of Tekoa. Being a scholar, he has been able to assemble an array of facts and instances which he cites in support of his statements, which makes them very convincing, and very discouraging as well.



JEN SHENG: *The Root of Life.*

*By Mikhail Prishvin. English Version by George Walton and Philip Gibbons. Foreword by Julian S. Huxley. Andrew Melrose, London.* 5s. net. $7\frac{1}{4}$ x $4\frac{5}{8}$; 157; 1936.

A man sick to death of war, leaves the battlefield strewn with dead and wanders into the wilderness to find a new soul. He meets Lu Wen, a Chinese philosopher who shows him the Root of Life, but he must wait many years until it develops before he may possess it. In the meantime he grows, conquering his hunting instinct when he looks into the beautiful flower eyes of Khu-a-lu the doe. Then comes the woman who is the very incarnation of the lovely Khu-a-lu, but she returns whence she came and he turns to the domestication of the almost extinct spotted deer.

If the usual sentimental nature book with animals thinking and speaking human emotions bores you, you will be delighted with Jen Sheng. Mikhail Prishvin has been compared with Grey Owl, but they are alike only in their deep understanding of wild animals, for Prishvin, with Russian seriousness finds a real philosophy in his experiences. He has given a vivid picture of the wilderness and beauty of Manchurian Russia, and a very interesting study of the life cycle of the spotted deer, which he sees with deep feeling and rare sympathy.

Jen Sheng is a small book, but with subtle simplicity sums up the very real philosophy of Soviet Russia's idealism, that "the power of the root of life lay not in its substance which also would soon be swept away, but in relation to cosmic time, and even to time which was still more vast."

TEACHING METHODS IN MEDICINE. *The Application of the Philosophy of Contemporary Education to Medical Schools.*

*By William D. Reid. William D. Reid, 510 Commonwealth Ave., Boston.* $1.00. 9 x 6; 111; 1936.

The expression "a born teacher" or even "a good teacher" can be applied by and large to but few university instructors in whatever department one may search. This little book, by the assistant professor of cardiology in the Boston University School of Medicine, makes a plea for the training of teachers, as such, of medical students. The average medical teacher is selected for a superior knowledge of his subject while little or no regard is given to his understanding of the methods of teaching. The author briefly discusses the psychology of learning; major, minor and general technics of teaching; and the application of these technics to medical education. He also gives a section on "selected topics" which are offered as samples in the application of the technics of teaching to medical subjects. Lists of suggested readings are given at the end of the chapters and the volume is indexed.

TRANSFORMISME ET HÉRÉDITÉ. *Les Organismes et le Milieu Terrestre.*

*By César Porto. Librairie Bertrand, Lisbon.* 15 francs. 9 x $5\frac{1}{2}$; viii + 328 + 4 folding charts; 1936 (paper).

L'INSTINCT. *Ses Causes Physiques, sa Base Organique et sa Psychology.*

*By César Porto. José Corti, Paris.* 15 francs. 9 x $5\frac{1}{2}$; 288; 1935 (paper).

The author, former director of the technical schools at Lisbon, attempts to apply concepts of metaphysics and astrol-

ogy to, in the first case, evolution and heredity, and in the second, instinct. The second book is curiously enough a continuation of the first. There is much talk of cosmic rays, magnetic forces, etc., and even psychological factors, but the theories evolved and the hypotheses upon which they are based are not always clearly defined. The author apparently belongs to that group which either assumes that astrology has already been placed upon a scientific basis, or is trying to put it there. It is evident also that French is not his native language.

Neither book has a bibliography (indeed, a careful perusal revealed no reference to *any* other author) or index, but we opine they will not be greatly missed.